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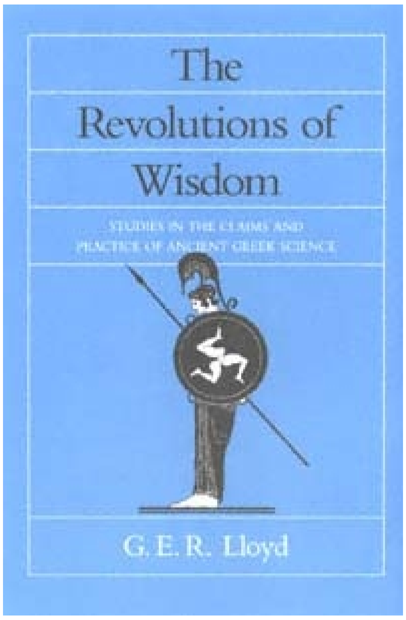

The Revolutions of Wisdom

Studies in the Claims and Practice of Ancient Greek Science

G. E. R. Lloyd

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Preface

The particular challenge of an invitation to give the Sather lectures at the University of California at Berkeley is that of presenting to a sophisticated general audience some new ideas, arguments, or line of inquiry concerning some aspect of Greco-Roman antiquity that the lecturer—given carte blanche on the choice of subject—deems to be of interest and importance. Having worked for many years on topics to do with the inauguration of scientific investigations in the ancient world, I naturally decided, when I was honoured to receive the invitation to give the 1983–84 lectures, to concentrate on this general area, and to focus in particular on one fundamental and extremely difficult question that this topic presents: to what extent, and in what way, was there a breakthrough in the understanding of nature and on the question of how to go about securing such an understanding? It has in the past often been assumed that the answer is obvious: insofar as these investigations merit the title of science,

of course it goes without saying that they must mark a departure from traditional, pre-scientific patterns of thought. Yet specifying precisely in what that departure consists turns out to be more complex than is generally imagined. The problems concern the evaluation not only of the explicit aims, methods, and ideals of ancient investigators—whether natural scientists, philosophers, or doctors—but also of their actual practice: just how far are their aims and methods original? Just how successful were they in living up to their expressed ideals in the actual investigations they carried out?

In taking up this challenge I first concentrated on producing a set of lectures that sketched out lines of argument on key issues but inevita-

bly kept the supporting documentation and illustration to a minimum. To meet the demands of supplying such documentation involved considerable expansion of the text of the lectures, although the overall strategy of this book still corresponds closely to that of the lectures as delivered. Much of the documentation is confined to the extensive notes, printed here at the foot of the page so that, while the argument in the text can be read independently, the reader can see at a glance where there are supplementary points and questions to be pursued. As I have attempted to bring to bear ideas that derive from my own reading in many different fields —the philosophy and sociology of science, social anthropology, Oriental studies, as well as the scholarly literature on Greco-Roman antiquity—I have provided the work with a full, though still far from exhaustive, bibliography. I have done so not just from the obligation to acknowledge my sources, but also in the hope that those from different disciplines who may be interested in the problems raised here may have an introduction to some of the relevant literature from other cognate fields.

Many friends and colleagues have been kind enough to read and comment on drafts of this work. I owe special debts of thanks first to Anthony Bulloch and Linda Coleman, who gave me their detailed and most perceptive reactions to drafts of the lectures, and also to Giovanni Ferrari, whose constructive and critical reading of the typescript of the whole book has saved me from many mistakes and enabled me to make many improvements. I owe much to Andrew Barker for his advice on music theory, to Simon Goldhill and Mary Hesse on metaphor, to John Ray on Egyptological issues, to Andrew Stewart on art historical problems, and to Jack Goody, Caroline Humphrey, and Alan Macfarlane especially on anthropological questions. Many others too have helped with comments on particular points or on the arguments of whole sections: Myles Burnyeat, Richard Gordon, Peter Khoroche, Wilbur Knorr, Martha Nussbaum, Thomas Rosenmeyer, Malcolm Schofield, David Sedley, Richard Sorabji, Gregory Vlastos. On many different occasions I have had the benefit of questions and comments from audiences at lectures and seminars based on this material. My graduate seminar in ancient philosophy in Cambridge in 1983 proved one of my most

consistently tough and creative audiences, and I learnt

much also in Cambridge at the History and Philosophy of Science and Social Anthropology seminars, in London at University College and at Chelsea College, at the Queen's University at Belfast, and in North America at the University of British Columbia, at Stanford, and at St. Mary's College of California. Most notably the comments from the audiences at my lectures and graduate seminars at Berkeley itself throughout the spring semester of 1984 stimulated me to clarify, justify, or modify my positions.

The hospitality accorded to Sather lecturers is legendary, and in reality the kindness of Leslie Threatte, and of all his colleagues in the Department of Classics, to myself, my wife and my family was indeed overwhelming. We were entertained, guided, instructed, and amused, with generosity, warmth, tact, and imagination, introduced by turns to Californian Nature and to Californian Culture and enchanted by both. No expressions of gratitude can begin to be adequate: our thanks, nevertheless, to all our hosts, and especially to Bill and Deidre Anderson, Esperance and Jock Anderson, Alan Code, Alan and Carolyn Dundes, Crawford Greenwalt, Jr., Mark Griffith, Eric Gruen, John Heilbron, Sylvia Lark, Kay and Tony Long, Don Mastronarde, Charles Murgia, Michael Nagler, Lila and Tom Rosenmeyer, Allan and Annie Silverman, Connie and Ron Stroud, Leslie Threatte.

Finally I wish to express my thanks to the officers of the University of California Press, and especially to Doris Kretschmer and to Mary Lamprech, for their exemplary efficiency in overseeing all the stages of the production of this book.

G. E. R. L.
MAY 1986

Editions and Abbreviations Used

Except where otherwise stated, the fragments of the pre-Socratic philosophers are quoted according to the edition of Diels, revised by Kranz, *Die Fragmente der Vorsokratiker* (6th ed., 1951–52) (referred to as D.-K.); the works of Plato according to Burnet's Oxford text; the treatises of Aristotle according to Bekker's Berlin edition; and the fragments of Aristotle

according to the numeration in W. D. Ross, *Fragmenta selecta* (Oxford, 1955). Greek medical texts are cited, for preference, according to the *Corpus medicorum Graecorum (CMG)* editions. For those Hippocratic treatises not edited in *CMG* , I use E. Littré, *Oeuvres complètes d'Hippocrate* , 10 vols. (Paris, 1839–61) (L), except that for *On Sevens* I use the edition of W. H. Roscher (Paderborn, 1913). For those works of Rufus not in *CMG* , I use C. V. Daremberg and C. E. Ruelle, *Oeuvres de Rufus d'Ephèse* (Paris, 1879). Galen is cited according to *CMG* and Teubner editions (where these exist), but the reference is also given to the edition of C. G. Kühn (Leipzig, 1821–33) (K), which is also used for works neither in *CMG* nor Teubner; the later books of *On Anatomical Procedures* , extant only in an Arabic version, are cited according to the translation of W. L. H. Duckworth (D) (ed. M. C. Lyons and B. Towers, Cambridge, 1962).

Euclid's *Elements* are cited according to the edition of Heiberg, revised by Stamatis, 4 vols. (Leipzig, 1969–73) (HS), and the works of Archimedes according to Heiberg, revised by Stamatis, 2 vols. (Leipzig, 1972) (HS), with the third volume, containing Eutocius' commen-

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tary. Ptolemy's *Syntaxis* is cited according to the two-volume edition of Heiberg (Leipzig, 1898, 1903) (cited as H 1 and H 2); his *Tetrabiblos* according to F. Boll and A. Boer (Leipzig, 1942); his *Optics* according to A. Lejeune (Louvain, 1956) (L); and his *Harmonics* according to I. Düring (Göteborg, 1930) (D.). Porphyry's *Commentary on Ptolemy's Harmonics* is cited according to I. Düring (Göteborg, 1932) (D.).

Other Greek authors are cited according to the editions named in the *Greek-English Lexicon* of H. G. Liddell and R. Scott, revised by H. S. Jones, with Supplement (1968) (LSJ), though, where relevant, references are also provided to more recent editions, and Latin authors are cited according to the editions named in the new *Oxford Latin Dictionary (OLD)* , supplemented, where necessary, from Lewis and Short.

Abbreviations are those in LSJ and *OLD* , supplemented, where necessary, from Lewis and Short and with the following abbreviations of works of Galen: *AA (De anatomicis administrationibus), PHP (De placitis Hippocratis et Platonis)* .

Full details of modern works referred to will be found in the bibliography on pp. 337 ff. They are cited in my text and notes by author's name and publication date or dates. A double date is used to distinguish, where this has seemed relevant, the original publication from the revised or reprinted version used. Such works are listed in the bibliography by the first date but cited according to the second. Thus Kuhn 1961/1977 refers to the revised 1977 version of an article originally published in 1961; Scholz 1930/1975

refers to the 1975 translation of an article originally published in 1930.

The translations of Greek and Latin texts that I offer are in general my own but I have made extensive use of existing translations and in particular of the following: Chadwick and Mann 1978, W. H. S. Jones 1923–31, and Lonie 1981a for Hippocratic works; Dengler 1927, Hort 1916, Ross and Fobes 1929 for Theophrastus; Macran 1902 for Aristoxenus; Heath 1913 for Aristarchus; Spencer 1935–38 for Celsus; Temkin 1956 for Soranus; Toomer 1984 for Ptolemy; Duckworth 1962, May 1968, and Singer 1956 for Galen.

Chapter One—
The Displacements of Mythology

Jane Harrison thrilled to the dark shapes she thought she could discern behind the bright splendours of the masterpieces of Greek literature.**[1]** E. R. Dodds, in his preeminently distinguished contribution to the Sather series, began from the puzzlement that the Greeks had been thought to lack something of "the awareness of mystery" and "the ability to penetrate to the deeper, less conscious levels of human experience."**[2]** The irrational then and subsequently has been much pursued—in classical studies, in social anthropology, in philosophy, and in psychology—but has proved, predictably, an elusive quarry, escaping clear characterisation, let alone elucidation.

I shall certainly not attempt, in this set of studies, to reopen the whole of this vast and ill-defined dossier. My aim is a more limited one, with a narrower focus, though it is still perhaps ambitious enough,

[1] J. E. Harrison 1925, pp. 86f: "I mention these ritual dances, this ritual drama, this bridge between art and life, because it is things like these that I was all my life blindly seeking. A thing has little charm for me unless it has on it the patina of age. Great things in literature, Greek plays for example, I most enjoy when behind their bright splendours I see moving darker and older shapes. That must be my *apologia pro vita mea* ."

[2] On p. 1 of his 1951 Dodds wrote: "To a generation whose sensibilities have been trained on African and Aztec art, and on the work of such men as Modigliani and Henry Moore, the art of the Greeks, and Greek culture in general, is apt to appear lacking in the awareness of mystery and in the ability to penetrate to the deeper, less conscious levels of human experience."

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since it concerns the invention of the category—the ancient Greek category—of the rational. Acknowledging, but leaving to one side, much of the material that Dodds and others collected to illustrate the irrational in Greek culture at every period, I wish to focus attention on one of the citadels of presumed Greek rationality (presumed by many of them, as well as by some of us), namely, what they called the "inquiry concerning nature." My plan, broadly, is to investigate where, or if, it may be said to break new ground in the understanding of the world, and where, on the contrary, what it shares with its antecedents is more impressive than the points at which it diverges from them. The character of the "science" on offer in the ancient world is one of our targets, then, though less with a view to matching their science against ours (to vindicate or to undermine the claim that they were *doing science*) than to explore the complexities of ancient disputes and confrontations. We shall try to make some sense of some highly perplexing and challenging phenomena, though the perplexities and challenges are ones that the anthropologists, used to dealing with problems concerning the nature of "primitive thought," probably appreciate more fully than the majority of classicists.

We may take heart for the assault on Greek science from the realisation that scientific thought as a whole and, especially, the nature of scientific inventiveness have latterly come increasingly to be recognised as less translucent, more complex, puzzling, and problematic, than many of Dodds' generation and before took them to be.**[3]** But while that realisation makes our inquiry easier in one respect, in that it releases us from one set of preconceptions concerning the purity of the scientific enterprise, in another it makes it harder, since the very criteria of science are now more highly contested than ever. My chief concern in

[3] Among the fundamental contributions to this debate have been Popper 1935/1968, 1963, Quine 1953/1961, 1960, 1969, Kuhn 1962/1970, 1977, 1983, Feyerabend 1962, 1975, 1978, 1981a, 1981b, Habermas 1968/1978, 1971/1974, Hesse 1974, 1980, S. B. Barnes 1974, 1977, Putnam 1975a, 1975b, 1981, 1983, Lakatos 1976, 1978a, 1978b, Bloor 1976, Laudan 1977, Holton 1978, Van Fraassen 1980a, Newton-Smith 1981, Hollis and Lukes, edd., 1982, Hacking 1983.

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what follows is not directly with those current controversies in the philosophy of science, though I shall have occasion to join battle where they impinge on the assessment of ancient investigations. Rather, my main problem is the characterisation of those ancient investigations themselves, particularly in relation to *their* background. For while those who engage in them often make extravagant claims on their behalf (as also do some modern

commentators), just how far such claims can be sustained and just how far the principles and ideals they stated were implemented in practice will be among our major preoccupations. To put our problem in its most general terms: *Was* there a revolution of wisdom with regard to the understanding of nature? What *kind* of revolution was there?

In the chapters that follow I shall address some very general questions concerning the nature of Greek inquiry and speculation about the physical world, where I have chosen to concentrate not on such traditional topics as the experimental method but, rather, on certain characteristics that relate to, and reveal, the ancient investigators' own aims and ambitions, even their self-image, their theory of what they were doing and their actual practice and the matches and mismatches between the two. We shall consider the tension between tradition and authority on the one hand, and innovativeness on the other, broaching here issues in the wider social background to the intellectual changes with which we are concerned. We shall study the aggressions and bluff of dogmatism, but also—to set against that—the scrupulous avoidance of the dogmatic and the willingness to acknowledge failures and ignorance, and then again the turning of the anti-dogmatic into a conventional stance or even pose. We shall discuss the development, indeed the invention, of the category of the metaphorical, and again the tension between the desire to exclude this from, and its continued use in, the inquiry concerning nature. We shall examine the extent to which Greek science remained purely qualitative in character—where we shall discuss both the use of measurement and its abuse, that is, the mystifications involved in some appeals to it and to the quantitative. Finally we shall tackle the use of idealisations and simplifications, and again their abuse in the discounting or eliding of parts of what is there to be explained.

In this opening chapter I want to take certain concrete topics which will provide test-cases to illuminate the nature and the strength of the challenge, from the side of *logos* , to some traditional attitudes and patterns of belief. If we consider some phenomena that lie at or near the centre of most naive or sophisticated configurations of the irrational, we may be able to see to what extent the inquiry concerning nature offered an alternative to what had long been accepted. It is not that that inquiry was necessarily obliged to present any such alternative in relation to those phenomena; it may not even have been well advised to try to do so. Yet in the controversy between would-be science and the irrational, it is important to look at certain of the topics that are, on the face of it, among the *least* favourable to the rationalist takeover, not just at those areas where the triumphs of rationalism may seem predictable enough. It is important to do so to help to determine the character and the limitations of the wisdom that came to be offered from the side of *logos* .

Many of the phenomena discussed in *The Greeks and the Irrational* look

promising from the point of view I have specified, but among those that seem particularly so—in that they appear to offer some of the greatest problems for, or the maximum resistance to, any scientific takeover—are death, disease, madness, dreams, divination, and fate. These were the province of myth, religion, and ritual long before science and natural philosophy, and long *after* their first hesitant appearance in Greek thought. It was mainly through myth, in belief, and through ritual, in practice, that the Greeks, like others, responded to the facts of death and disease, for example—and it remained so, even after the inquiry concerning nature was some kind of going concern. yet to say the Greeks "responded to" natural facts through myth is not quite accurate. For myth is not, and does not aim to be, explicitly systematic and coherent.**[4]** I am not denying, of course, the findings of

[4] The point stands even though, to be sure, current theories on the interpretation of mythology can still hardly be said to provide a satisfactory framework for its understanding (see, for example, Lévi-Strauss 1958/1968, 1971/1981, Leach, ed., 1967, P. S. Cohen 1969, Smith and Sperber 1971). Thus despite, for instance, the claims of Van Riet 1960, p. 63, the sense in which sys-tems of myth can be said to constitute some kind of protometaphysics is only a very attenuated one and this may obscure more than it illuminates: for the essential point of difference is that metaphysics is explicit, even if the point of similarity is that in the most general sense a "world-view" may be conveyed by myths or otherwise by implicit beliefs and attitudes as much as by self-conscious philosophical statements. The flexibility of myth, stressed by T. S. Turner 1977, 1980, for instance, is both its strength and its weakness. P. Smith's statement, 1973, p. 77, that "[myths] taken as a whole, aim not so much to define the real as to speculate upon its latent potentialities; not so much to think something through as to walk the boundaries of the thinkable," suggestive as it is, is made, rather, from outside the boundaries of myth itself. His equally suggestive remark, p. 86, that "when *logos* recognizes *mythos* as such, and so deprives it of its efficacity, at the same time it takes over its place and becomes a new working myth," and his tentative "it may be that if one is to do full justice to science, one must acknowledge the portion of myth it has in it" (cf. also Derrida 1967/1976, 1972/1981, 1972/1982) may be said to encapsulate the *problems* explored here, the sense in which what replaces myth, in ancient Greece, was or was not *just* more myth, and the difference that that recognition made.

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structuralism, which has decoded remarkably coherent messages in groups of myths, even whole mythologies. But those messages, as structuralism itself insists, remain implicit, below the surface. On the surface, the intelligibility provided by myth is metaphorical, both in the sense that it is of the nature of metaphor and in the sense that it is a qualified intelligibility.**[5]** Myth does not, in any case, normally attempt to give the kind of direct answers to questions that ordinary practical experience is used

to and demands. To be effective, myth must work below the surface, while on the surface the appearance is often of inconsistencies, of a lack of coherent unity. The encoded messages are vulnerable to question, to challenge, and like books in Plato, they cannot answer back.**[6]** Equally, ritual comforts, in part, because in the already given and socially sustained patterns of behaviour it is simply the right thing to do. But again the vulnerability to the question "why?" is evident—as is shown by the dismay registered by an earlier tradition of ethnography when that question, pressed in the field, led with some

[5] See further below, Chap. 4, pp. 176ff. Chap. 6, pp. 285ff.

[6] The point, in Plato, applies precisely to the written word as opposed to the spoken exchange; see *Protagoras* (*Prt* .) 329a, *Seventh Letter* (*Ep* . 7) 343a, as well as famous texts in the *Phaedrus (Phdr.)* , 274b ff., especially 275d–e,277d–e (discussed from different points of view by, among others, Havelock 1963, cf. 1982, and Derrida 1967/1976, 1967/1978). I shall return, in Chap. 2, to some aspects of the issue of the labile or unstable nature of the oral tradition.

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inevitability to the—to *logos* unacceptable**[7]** —answers that "we have always done so," "this is laid down," "this is the way our forefathers did it."**[8]**

But if myth and ritual provide some imperfect means of responding, in various ways, to various manifestations of the apparently intractable or refractory in experience, what did the "response" of the new investigations into nature amount to? What business had they, in any event—to pick up my earlier question—with such phenomena as death, disease, and the others I listed, or how far did they abandon them; or should they have abandoned them, renouncing any claim to be able to provide alternative, and no doubt also imperfect, resources for a response? To be sure, that question, like my earlier questions, has to be unpacked even to begin to attempt an answer. What, in particular, were the problems to which solutions were required? What kinds of explanation were needed for what kinds of explananda? Are we dealing—to start with—with puzzles concerning the that (or what), the how, or the why?**[9]**

Death

The "that" of death (for instance), the fact that men die, cannot be treated as an unproblematic cultural universal.**[10]** We have only to reflect on beliefs in various modes of symbolic death to see that here, as so often elsewhere, there may be wide cultural divergences and substan-

[7] Cf. further below, Chap. 2, on the appeal to tradition as such as justification for a belief or practice.

[8] For some comments on this theme, see Horton 1982, pp. 239ff.; Sperber 1985, pp. 59f.

[10] See, for example, V. W. Turner 1964, p. 231.

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tial difficulties in matching actor and observer categories.**[11]***A fortiori* what counts as disease or illness and what as mental illness or madness vary strikingly between cultures. Yet so far as ancient Greek views of death go (the subject of another distinguished contribution to the Sather lectures),**[12]** a resolute acceptance *that* men die is strongly marked in Homer,**[13]** even if there is afterwards a shadowy existence in Hades, and even if some exceptional individuals escape that fate and achieve semi-divine status as heroes.**[14]** But acceptance of the brute fact of death gives no consolation for, indeed may even heighten, personal bereavement. That acceptance does not qualify, rather it lends resonance to, Achilles' anguished cry that he would rather be a bondsman on earth than rule among the dead.**[15]**

[11] Furthermore, the point that death may be viewed very differently as it affects the young and old is emphasised, for example, by Cassin 1981, p. 321.

[12] Vermeule 1979. Two recent collections of essays contain important discussions of Greek and Roman attitudes along with comparative studies of other cultures: Humphreys and King 1981, and Gnoli and Vernant 1982. Sourvinou-Inwood 1983 explores, in particular, changes in attitudes that take place at different periods in antiquity: cf. also Garzón Díaz 1981, Wankel 1983.

[13] See, for example, *Iliad (Il.)* 12.322ff., 18.115ff., 21.106ff., 24.525ff., and from a god's perspective, 21.462ff. The point has often been brought out forcefully, as by Rohde 1925, chap. 4; Guthrie 1950, pp. 305f; Sourvinou-Inwood 1983, pp. 34f. The centrality of the topic of death in the Homeric poems has recently also been stressed by Segal 1978.

[14] See, for example, *Odyssey (Od.)* 4.561ff. (Menelaus), 11.300ff. (Castor and Polydeuces), 11.601ff. (Heracles). Some individuals are, of course, subject to exemplary punishment: see especially *Od.* 11.576–600 (Tityus, Tantalus, Sisyphus).

[15] *Od.* 11.488ff. How far, on this or any other of the issues germane to our discussion, Homer should be taken to represent common attitudes is, to be sure, highly problematic, but the influence and prestige of the Homeric poems in the fifth and fourth centuries insure at least their relevance to our understanding of the background to natural philosophical speculation. Aspects of the themes of the transience of human life, human helplessness, and the preponderance of evil, expressed, for example, at *Il.* 6.145ff., 21.464ff., 24.527ff., *Od.* 18.130ff., and in Hesiod, e.g., *Works and Days (Opera, Op.)* 101ff., are reiterated in early lyric and in tragedy, e.g., Solon 1.35ff. (Diehl), Mimnermus 1 and 2 (Diehl): the theme that it is better not to have been born at all, found, for example, in Theognis 425ff., Bacchylides 5.160ff. (Snell-Maehler) and Sophocles *Oedipus Coloneus* 1224ff., reappears in a particularly emphatic statement in Aristotle's lost dialogue the *Eudemus* fr. 6 (Ross).

On the how of death, Greek physics eventually had, as usual, a multitude of theories to offer. Yet they provided little understanding and no reassurance. There was Aristotle's suggestion, for example, that death is the extinction of the vital heat, which may take place, he believes, either from cold or from an excess of heat.**[16]** That theories that appeal just to the hot, the cold, and the like are quite inadequate had already been argued in the Hippocratic treatise *On Ancient Medicine* . There the writer criticises those who use such newfangled "hypotheses" in part on the grounds that to do so is to narrow down the causal principles of death and disease.**[17]** What is needed, he believes, is a more complex account, taking into consideration all the manifold powers in the body and their combinations.**[18]** Again even Plato had a suggestion to make on the subject in the *Timaeus* , namely, that the material cause of death is a deterioration in the structure of the atomic triangles that constitute the physical elements of which the body (and everything else) is made.**[19]**

To be sure, each of those, and many other, hardheaded naturalistic explanations entailed the denial of the *literal* truth of Hesiod's mythology of death as presented in the myth of the metals in the *Works and Days* , with its complex counterpoint on the way each race meets its end.**[20]** Those of the Golden Age are as if overcome by sleep; those of the Age of Silver, who remain children for a hundred years, are "hidden" by Zeus and become the blessed ones of the underworld; the Bronze Age race destroy themselves; some from the Age of Heroes go

[16] See, for example, *De juventute (Juv.)* 469b18ff., 21ff., *De respiratione (Resp.)* 478b22ff., 479a32ff. Contrast *Ethica Nicomachea (EN)* 1115a26, where Aristotle recognises that death is the most fearful thing there is.

[18] See, for example, *VM* 14, *CMG* 1.1.45.26ff., 15. *CMG* 1.1.46.27ff., 22,

CMG 1.1.53.1ff.

[19] *Timaeus (Ti.)* 81b–e.

[20] *Op.* 109ff., on which see, for example, Kirk 1970, pp. 233ff., J.-P. Vernant 1983, pp. 3ff., 33ff.

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to the Islands of the Blest; and Zeus will destroy the last Age of Iron when men are born grey-haired.**[21]** Again, theories of the physics of death were not compatible with a *literal* reading of Plato's own myth, in the *Politicus* , of the age of Cronos—the anti-cosmos when time flows in reverse**[22]** —while they had no comment to make on the values implicit in the ideology of the "beautiful death"—the death while young, in battle, securing lasting fame.**[23]** No prosaic naturalistic account of the how of death had, of course, anything to offer on the why, nor on how we as mortals should live with our mortality. They offered nothing to replace the lesson obliquely taught by Hesiod's myth: we must realise that, since we are born in the Age of Iron, there is an imperative upon us to accept death, along with toil and pain.

Such comfort as was on offer from the philosophers in the classical period, at least,**[24]** came principally from a very different quarter, from the essentially religious belief in the immortality of the soul found first in the Pythagorean tradition, then in Plato and others.**[25]** Yet that was certainly not *science* replacing earlier attitudes or patterns of belief.

[21] See *Op.* 116, 137ff., 152ff., 170ff., 180f.

[22] *Politicus (Plt.)* 268e ff., especially 270c–e referring to periodic destructions of the human race and the reversal of aging, with the old becoming young. Compare the discussions in Herter 1958, Rosen 1979–80, and especially Vidal-Naquet 1975/1986.

[23] See especially Loraux 1981/1986, pp. 98ff., 1982, pp. 27ff.; J.-P. Vernant 1982, pp. 45ff.; cf. Dover 1974, p. 229; Sourvinou-Inwood 1983, p. 43.

[25] Of course it was not just from among the philosophical writers that comfort of this kind was on offer, but also, as early or earlier, from within the growing and altering religious traditions, notably with the development of mystery religions: see Burkert 1977/1985, chap. 6, pp. 276ff., cf. Nilsson 1957, for Hellenistic continuations.

On the contrary, in Plato at any rate, it was often what was now self-consciously recognised as *myth* doing so. The substance of the eschatological accounts in the *Gorgias, Phaedo, Republic* , and *Phaedrus* is accepted by the character Socrates, and no doubt by Plato, as true or at least as like the truth:**[26]** there would presumably be no wavering on the underlying principle of rewards and punishments, the ultimate justice of the regulation of the universe. Yet on each occasion the account is expressly said to be myth, or its status is otherwise undercut.**[27]** In the *Phaedrus* , for instance, Socrates begins by offering a "demonstration,"

ἀπόδειξις

, though it is one that will be "untrustworthy to the clever, but trustworthy to the wise."**[28]** But after some oracular pronounce-

ments on the immortality of the soul, he turns to describe its character

(ἰδέα)

with the disclaimer: "What kind it is [must be said] to be in every respect and in every way a matter of a divine and lengthy exposition: but what it resembles, of a human and lesser one. Let us therefore speak in that way."**[29]** Evidently here and elsewhere what we are given by Plato in the eschatological myths is what he believes, not what he believes can be established to the last detail by exact dialectical argument.**[30]**

Disease

This first round is, then, no real contest: there is no scientific takeover, no real engagement in the classical period between the study of nature and what myth and religion afforded, whether they did so self-consciously or otherwise, and whether as developed and presented by philosophers or otherwise. But there is more to be said on what might otherwise seem the similar topic of disease, and more on mental illness or madness. In the case of disease, too, the study of nature offered no answer to the naive question of *why* disease in general should occur: Why do we not live in continual perfect health? Yet it certainly effectively, and in some cases deliberately, blocked any move to explain diseases—both particular types of diseases and

individual incidences of them—by invoking divine or supernatural agencies.**[31]**

[30] It is because—unlike myth—metaphysics is explicit that it can be, as Plato's was, explicit, in particular, about its own limits, where myth has to take over. That *logos* is often to be contrasted with *mythos* is not of course to deny that the relationship between the two is an intricate and far from straightforward one. We shall be returning to consider other aspects of this with regard to Plato in Chap. 3 at nn. 115ff. and Chap. 4 at nn. 30ff. Cf. Detienne 1981/1986, Brisson 1982, Ferrari forthcoming.

[31] For what follows on the Hippocratics, cf. G. E. R. Lloyd 1979, chap. 1. The turning away from the belief in the supernatural causation of diseases and from the expectation of the efficacy of supernatural aid in combating them can also be illustrated from outside the medical writers, in, for example, Thucydi-des 2.47 and 53—though such beliefs and expectations persisted in many quarters. In the Hellenistic period a large part of Epicurean natural philosophy was to be devoted to excluding divine or supernatural agencies from the explanation of natural phenomena: cf. further below, n. 163, and Chap. 3, n. 239, in the context of Epicurean appeals to the principle of plural explanations of obscure phenomena.

To see this in the right perspective is more complicated than might appear. First it is as well to stress that not *all* physical ailments and disabilities were deemed by the ancient Greeks—or have been by anyone else—to be the products of divine or demonic forces. Medical anthropologists have, to be sure, only comparatively recently begun to insist that there is much more to the map of most societies' beliefs about physical ailments than the parts that have generally received most attention in the ethnographical accounts, that is, those that relate to the severest diseases and the most dramatic ones, such as epilepsy.**[32]** For many minor ailments, as it might be the common cold, minor stomach upsets, bruises, or bunions, many societies have no recourse to supernatural explanation. Homer has no occasion to talk about the common cold. But apart from the fact that there are many straightforward accounts of wounds and lesions caused by men in battle**[33]** there is an important contrast between the plague sent by Apollo in *Iliad* 1**[34]** and references to diseases that are not directly attributed to a god, such as, for example, the "long disease" contrasted with the arrows of Artemis at *Od.* 11.172.**[35]**

However, this is not to deny that notions that diseases are often sent by the gods or that diseases are themselves semi-divine creatures stalk-

[32] See, for example, G. Lewis 1975, pp. 196ff., 248ff.

[33] Those wounds are caused by men, even though the success or failure of a blow may often be ascribed in addition or in part to a god—as with other phenomena that have been discussed under the rubric of "double determination," where both a divine and a human explanation are invoked: see, for example, Dodds 1951, chap. 1.

[34] *Il.* 1.43ff. This has, of course, a particularly important role in initiating the action of the epic. But when there is less at stake for the purposes of the narrative, the darts of Apollo or Artemis are still often invoked as causes of death or disease, for example at *Il.* 6.428, 19.59, 24.758f., *Od.* 3.279f., 5.123f., 11.324, 15.478f., 18.202ff., 20.61ff., and cf. also 9.411.

[35] Cf. *Od.* 11.200f. See also *Il.* 13.666–70, *Od.* 15.407f.

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ing the earth**[36]** are widespread and deeply entrenched in archaic times and after.**[37]** Whereas the fact of death in general (as opposed to the ideology) did not, on the whole, need demystifying in Greece,**[38]** the topic of diseases undoubtedly did; and in a remarkable, unprecedented move, some of the Hippocratic writers made a deliberate and self-conscious bid to secure such a demystification, even if it was one accompanied by its own elements of paradox and obfuscation.

First as to the elements of paradox. The move consisted in deeming all diseases natural, not subject to divine intervention, divine only in that the whole of nature is.**[39]** They all have natural causes, a

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, as the author of *On the Sacred Disease* puts it.**[40]** But if natural, in that not demonic, diseases are also unnatural, as contrary or hostile to the nature of the organism. It is *necessary* for the human being to grow old and to die, in the *natural* course of events.**[41]** But it is not necessary, not part of what it is to be a human being—even though it is no doubt usual**[42]** —to suffer from diseases, from fever or dysentery or pneumonia. This was no doubt not just usual, but also often beyond the control of the Hippocratic physician. The notion of "nature" that the Hippocratic writers work with is one that must insist that the

[36] Apart from the texts cited above, n. 34, see especially Hesiod *Op.*

102ff., cf. 242ff.

[37] Some of the primary texts are set out in G. E. R. Lloyd 1979, pp. 29ff. and n. 98, where reference is also made to the extensive secondary literature on the topic.

[38] A distinction is to be observed here between attitudes to death as such in general, and attempts to explain or account for the death of this or that particular individual.

[40] See *Morb.Sacr.* 1 (L) 6.352.1ff., 2 (L) 6.364.9ff., 18 (L) 6.394.9ff., especially.

[41] For one passage outside the medical literature that distinguishes natural from unnatural death, see Plato *Ti.* 81d–e, where natural death is said to be pleasant rather than painful.

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causes of diseases are physical ones but allow for a norm or model of a healthy living body, by reference to the *nature* of which diseases can be assessed, and to secure a return to which the doctor exercises his best efforts. The doctor must help nature to effect its own cure, even though the disease to be cured is itself natural and has *its* nature.**[43]**

Health and disease are both thereby located in the domain of what is, the doctors claimed, in principle investigable. The success of the naturalistic framework thus provided for understanding depends, however, on the pathological theories being accurate or at least helpful, on the diagnoses being reasonable, and on the treatments being at least to some extent apparently effective. We shall be returning in chapter 3 to discuss the arbitrariness and dogmatism of many of the theories proposed. For our present purposes I may simply note that it is characteristic of a good deal of Hippocratic medicine that the writers overstate their cases. From the observation that bile and phlegm may be pathogenic substances, there were those who leapt to the conclusion (as in the treatise *On Affections*) that *all* diseases came from bile and phlegm.**[44]** From noticing prominent changes in temperature

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and humidity in many patients, some became convinced that the hot, the

cold, the wet, and the dry are themselves the causes or bring about changes in the elements or humours in the body and thereby give rise to diseases.**[45]** Yet against the view that those four opposites are the sole causes of death and disease one can range the treatise *On Ancient Medicine* , which I have already mentioned, the author of which protests that that is to narrow down the causal principles. He objects to those who invoke these opposites on the grounds that they are trying to base medicine on unverifiable postulates where "it would not be clear either to the speaker or to his audience whether what was said was true or not"**[46]** —even if this author himself then goes on to develop some physiological and pathological theories of his own that are, we might say, not much less speculative than those he dismisses.**[47]** The point need not be elaborated further, since the elements of bluff in many Hippocratic theories of disease are obvious enough. We clearly need to suspend disbelief, if not our critical judgement entirely, when

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we are solemnly told, as by the author of *On Breaths* , that air is the cause of every illness,**[48]** or, by the author of *On Regimen* , that fire and water ultimately are.**[49]**

On the question of diagnosis and treatment, however, if we turn to those treatises that stay closer to clinical practice there is much that is sensible and sober and much that is perceptive, alongside much that perhaps needed no special Hippocratic skill to perceive. The why and how of particular types of diseases have become a possible subject of study, even if progress towards adequate answers was slight. Deploying pathological notions that were, in many cases, entirely superficial, research was generally directed at what we should call symptoms, and the causes often remained undiscovered, and not just when the causes that modern medicine would invoke depend on severely modern conceptions such as that of microorganisms. Yet research is the right term for the sustained effort to obtain a typology of diseases, to chart their progress and outcome, to establish correlations between apparently relevant factors, to move towards hesitant epidemiological generalisations.

Epidemics 1 conveys the tone: "Painful swellings near the ears which accompanied fevers did not always subside nor suppurate when the fever was resolved with a crisis, but they were relieved following bilious diarrhoea or dysentery or by the formation of sediment in the urine, as happened in the case of Hermippus of Clazomenae."**[50]** Or again:

> In this constitution [that is, during this epidemic] there were four signs especially that betokened recovery: a considerable nose-bleed, a copious discharge of urine with a lot of favourable sediment, bilious disorders of

[49] The imbalance of food and exercise that provides the chief theme of the discussion in *Vict.* is itself interpreted in terms of the relationship between the powers in the body which is constituted by fire and water: see *Vict.* 1.2–4 especially (L) 6.468.6ff., 472.7ff., 12ff., 474.8ff. (cf. below, Chap. 3 n. 39).

> the belly at the right time, or dysentery. In many cases the crisis was not reached with the appearance of just one of the signs described, but in most cases all were experienced successively and the patients seemed to be in great distress: but all who experienced them recovered. . . . I know of no woman who died in whom one of these signs had appeared properly. For the daughter of Philo, who had a violent nose-bleed, dined rather intemperately on the seventh day: she died.**[51]**

We are evidently far from the world of Apollo sending the plague or of Hesiod's diseases randomly roaming the earth.**[52]** Interestingly enough, however, a feature that provides both a link and a contrast with earlier patterns of thought is the residual moralising tone of some Hippocratic comments on the causes or predisposing factors to diseases. *Epidemics* 1.9 gives a list of the types of persons who died in a particular epidemic. They include: "boys, young men, men in the prime of life, those with smooth skins, those of a pallid complexion, those with straight hair, the black-haired, the black-eyed," and so on, but also "those who had been given to reckless and loose living."**[53]**

One of the case histories in *Epidemics* 3 begins: "Nicodemus took a fever at Abdera as the result of sexual indulgence and drinking,"**[54]** and another: "at Meliboea, a young man who had been heated for a long time as the result of drinking and much sexual indulgence, took to his bed"**[55]** (unlike Nicodemus, who had a crisis and recovered, the youth at Meliboea died). Immorality or at least intemperateness leads here (as in archaic thought) to sickness. Yet the difference should be remarked: it is not Apollo who strikes you down for offending him or his priest. The damage you do to yourself has no supernatural cause, only a natural one for which you are yourself solely responsible—namely, your own self-indulgence.**[56]**

On the topic of treatment, similarly, Hippocratic prophylactic recommendations were generally of more help than the treatments they prescribed for their patients once sick. Those treatments were often ineffectual (though that did not prevent some extravagant claims being

made on their behalf)**[57]** and they were sometimes more dangerous

[57] There were, however, those who recognised the ineffectuality of particular remedies or who admitted to an inability to cure particular diseases: see below, Chap. 3, pp. 124ff.

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than the condition they were used to remedy, whether it was a matter of the drugs employed (which included hellebore, Spanish fly, black nightshade, and a variety of compounds of arsenic)**[58]** or of the surgical procedures used (such as succussion upside down on a ladder,**[59]** forcible straightening on the bench,**[60]** cauterisation, trepanning). The value and importance of plain living, simple foods, regular exercise are stressed in many works, though again, to get the record straight, one must add first that there were doctors who went to extremes: Herodicus, who is famous from Plato, is said to have killed his patients by excessive exercise,**[61]** and there were others whose idea of simple

[61] Plato *R.* 406a–c. At *Epid.* 6.3.18 (L) 5.302.1ff., Herodicus is said to have killed patients suffering from fever by prescribing runs, wrestling, and steam baths.

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food was a starvation diet, for which there was even a technical term,

λιμοκτονία

.**[62]** Moreover, as happens so often in Greek medicine, a simple point was subjected to massive theoretical over-elaboration. One of our Hippocratic treatises is entirely devoted to the subject of its title, *A Regimen for Health* . Although the final chapter ends with the laudable sentiment that "an intelligent man ought to reckon that health is man's most valuable possession and learn how to gain help in illnesses by *his own* judgement,"**[63]** the work as a whole sets out some very elaborate recommendations about foods, exercises, emetics, and enemas that would have gladdened the heart of any ancient hypochondriac and that also implicitly laid claim to much esoteric learning on the subject.**[64]**

The topic of physical illnesses offered one of the clearest openings for the rationalist takeover. There are plenty of signs of the *hubris* of Greek rationalism in the Hippocratic treatises, as also of its tendency to run to

excess. Yet one of the strengths of the new conceptual framework they present, and one of its originalities, lies in its absolute, un-

[64] See, for example, *Salubr.* 5 (*Nat.Hom.* 20), *CMG* 1.1.3.212.20ff. ("After bathing in warm water, let the patient first drink a cotyle of neat wine: then let him eat food of all sorts and not drink either with the food or after it, but wait enough time to walk ten stades; then mix three wines, dry, sweet, and acidic, and give him these to drink, first rather neat and in small sips and at long intervals, then more diluted, more quickly, and in larger quantities.")

compromising character. The assumptions to be made (about the naturalness of diseases) and the way forward in research are confidently sketched out, even if the elements of promise are greater than those of fulfilment, for in practice delivery fell short both in the matter of understanding and in that of control—that is, the cures achieved.

Madness

Mental illness posed problems for the rationalists that were at points importantly different from those of physical illnesses.**[65]** While physical sickness was never exactly celebrated (though the case of Philoctetes illustrates that it could be viewed with awe),**[66]** there were what Dodds called, after Plato, the *blessings* of madness,**[67]** especially the gift of prophecy and the inspiration of poetry. There is no need to rehearse the rich variety of phenomena to which Dodds drew attention other than to recall that they included not just the star examples of the statement of the power of Dionysus in the *Bacchae* and the exceptional recognition of the positive manifestations of madness in Plato's *Phaedrus* , but much else besides in Greek religious belief and practice as well as in Greek literature.**[68]** The question I wish to address is, rather, the following: in the face of these proofs of the hold, so to speak, of madness

[65] Two recent books, B. Simon 1978 and Pigeaud 1981, provide helpful general discussions of many aspects of Greek attitudes towards mental illness.

[66] To be sure, Philoctetes is a case where physical condition, moral persona, and position in relationship both to the gods and to human society are inextricably interwoven.

[68] See Dodds 1951, chap. 3, which often builds, as he acknowledges, on Rohde 1925 especially.

on the Greek imagination, how did the would-be rationalists fare? Among those would-be rationalists the pre-Socratic natural philosophers were the first in the field,**[69]** but I shall concentrate once again on the fuller material available in the medical writers.

Their ambition to naturalise mental illness as well as physical, to treat it both conceptually and in practice no differently from physical, is clear, but we must ask with what success they did so, and at what price. First some of the material that is important to us, much of it less familiar now than the texts on which Dodds focused, should be set out, and we may begin with another of the case histories from the *Epidemics* , the account of a condition that was evidently taken to be at least in part psychological in origin:

> A woman at Thasos became morose as the result of a grief with a reason for it, and although she did not take to her bed, she suffered from insomnia, anorexia, thirst, and nausea. . . . Early on the night of the first day, she complained of fears and talked much; she showed despondency and a very slight fever. In the morning she had many convulsions; whenever the frequent convulsions intermitted, she talked at random and used foul language; many intense and continuous pains. On the second day, condition unchanged, no sleep, higher fever. Third day: the convulsions ceased but coma and lethargy supervened followed by renewed wakefulness, when she kept leaping up and losing control. There was much random talk and high fever. That night she sweated profusely all over with warm sweat. She lost her fever and slept, becoming quite lucid and reaching the crisis. About the third day the urine was dark and thin and contained suspended matter, for the most part round particles, which did not sediment. Near the crisis, copious menstruation.**[70]**

As this and many other examples show, Hippocratic accounts of symptoms move in a continuous gradation from thirst and nausea,

[69] Thus according to Caelius Aurelianus *Morb.Chron.* 1.145, the followers of Empedocles explained one kind of madness as a disorder of the mind arising from a bodily cause, though another arises from a purification of the soul.

through anorexia and insomnia, to despondency and depression, or from

high fever, to the delirium that so often accompanies it, to the patients being out of their minds, or from twitching and convulsions, to agitation and anger, to hallucinations.**[71]** As we noted before, attempts are made to establish correlations. For example, the third constitution in *Epidemics* 1 chap. 9 states: "High fever attended the start of the illness along with slight shivering fits, insomnia, thirst, nausea, slight sweating about the forehead and over the clavicles (but in no case all over), much random talk, fears and despondency, while the extremities such as the toes, and especially the hands, were chilled."**[72]** The doctors were concerned to collect cases of cold toes along with those of fear and despondency: all formed part of a total homogeneous epidemiological picture.

The strength of the Hippocratic approach to madness lies, as before, in its naturalism.**[73]** There is no question of any of these writers

[73] A naturalistic attitude towards madness and physical explanations of its origin can be illustrated from non-medical literature in the fifth and fourth centuries in, for example, Xenophon *Memorabilia (Mem.)* 3.12.6. But passages in Herodotus, for instance, illustrate how such an attitude may still be combined with traditional beliefs about the possibility of divine intervention:in 3.33 the possible reasons for Cambyses going mad are either that he offended Apis or that it came about because he suffered from the sacred disease (evidently treated here as primarily a bodily condition), and cf. the alternative accounts reported on Cleomenes' madness, 6.75ff. and 84. Cf. G. E. R. Lloyd 1979, pp. 29ff.

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thinking of madness as the result or the manifestation of

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no question, here, of any concessions to the blessings of madness. Madness is mental illness, and mental illness, like any other, is investigable and treatable. Yet the assumptions that are made are considerable. There is no sign of any realisation of the particular difficulty of specifying *what* mental illness is, what it takes for a patient to be mad. Foul language and random talk (as the case cited shows), even (as other cases do) "much talking, laughter, and singing"**[74]** are signs of abnormality; so too is loss of memory (not specified further):**[75]** so too, on some occasions, is silence.**[76]** The doctor is confident that the patient was merely babbling, or was unnaturally silent. He is confident, too, that he can tell the difference between depression arising from a distinct external stimulus,

μετὰ προφάσιος

,**[77]** and straight depression.

While the resolute matter-of-factness robs of its purchase any attempt to glorify madness, there seems no recognition that some modification in approach when dealing with mental illness might be called for. Treatments are, in any case, not often discussed in the case histo-

[75] E.g. *Epid.* 3 case 13 of the second series (L) 3.140.7.

[76] E.g. *Epid.* 3 case 15 of the second series (L) 3.142.8, 146.5. Elsewhere, at *Aph.* 2.6 (L) 4.470.17f., insensitivity to pain is taken as a sign of mental disorder; at *Aph.* 5.40 (L) 4.544.16f., it is said to be a sign of madness when blood congeals around a woman's nipples; at *Aph.* 5.65 (L) 4.558.7f., madness is said to follow when the swellings that accompany wounds in the front of the body suddenly disappear; at *Aph.* 6.21 (L) 4.568.7f., varicose veins and haemorrhoids are said to bring an end to madness; at *Aph.* 7.5 (L) 4.578.14, it is said to be a good sign when madness is followed by dysentery, dropsy, or an ecstatic state. In *Aër.* 7, *CMG* 1.1.2.36.12f., when the effects of drinking stagnant water are described, pneumonia and madness are said to attack young people in winter.

[77] See *Epid.* 3 (L) 3.134.3, cited above n. 70.

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ries in the *Epidemics* , but one theme is not reassuring. Several of the patients (as in the case cited) "lost control"—

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—and some were clearly actively restrained.**[78]** We do not know what kinds of restraint were attempted by the Hippocratics, nor how severe, nor in what precise circumstances, but there is no need to agree with the more extreme themes developed in some modish modern psychology**[79]** to see that this has an ominous ring. We hear from later writers such as Celsus in the first century A.D. and Caelius Aurelianus in the fourth that some medical theorists advocated violent and gruesome "cures" for

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. These included chaining the patients, drugging them, starving them, keeping them in the dark, making them drunk, and flogging them, and though Caelius is outspoken in his criticisms of most of these,**[80]** Celsus gives some of them

[80] *Morb.Chron.* 1.144ff., 171ff. In presenting what he calls the Methodist account, which includes, nevertheless, criticisms of some early Methodist doctors, Caelius Aurelianus is no doubt drawing mainly on Soranus—though here and elsewhere it would be rash to assume he is simply translating Soranus (cf. G. E. R. Lloyd 1983a, p. 186 n. 258). Other less violent remedies recommended by other physicians but criticised by Caelius include cooling substances (the idea that madness results from heat is ascribed to Aristotle andDiocles, 1.173), music, and inciting the patient to fall in love. The treatment Caelius himself recommends is set out at 1.155ff. It involves the avoidance of disturbance or excitement, the use of massage, warm fomentations, a reduced diet, and varied food, then, in the convalescent phase, such activities as having the patient deliver speeches, setting problems appropriate to his profession or craft, having him read aloud from texts with mistakes in them to keep him alert, and letting him see mimes or tragedies depending on whether he is gloomy or frivolous. Yet Caelius admits that the patient may have to be bound, though he adds that care should be taken not to injure him (1.157) and he allows venesection, scarification, the application of leeches, and even the use of rapidly dripping water to induce sleep (1.158–61).

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a limited endorsement.**[81]**

When we turn back to the Hippocratic treatises that attempt *theories* of madness, the impression they give is very much one of their authors whistling in the dark. Dealing with different kinds of mental disturbance, *On the Sacred Disease* , for example, states: "The brain may be corrupted both by phlegm and by bile, and you can distinguish the two types of disorder thus: those whose madness results from phlegm are quiet and neither shout nor make a disturbance; those whose madness results from bile shout, play tricks, and will not keep still, but are always up to some mischief. Such are the causes of continued madness. But fears and frights also occur from a change in the brain. Such a change happens when it is warmed and that is the effect bile has when, flowing from the rest of the body, it courses to the brain along the blood-vessels. Fear persists until the bile runs away again into the blood-vessels and the body, and then it stops. Pain and nausea are the result of in-opportune cooling and abnormal consolidation of the brain, and that is the effect of phlegm, and the same condition is responsible for loss of memory."**[82]**

[81] *De medicina (Med.)* 3.18 (*Corpus medicorum latinorum* [*CML*] 1.122.14–127.15). On whether the patients should be kept in the dark or in the light, Celsus says that neither rule is invariable; the doctor should try out both and see how the patient responds. He criticises the use of starvation in some circumstances, but when discussing the most severe and prolonged type of madness allows that in certain cases the patient should be coerced by starvation, chains, and flogging, on the grounds that it is good for him to

be frightened: 3.18.21, *CML* 1.126.27ff.

This treatise has a well-deserved reputation for its often well-directed attacks on the ascription of epilepsy to divine intervention and on the charlatanry of those who claimed to be able to cure it by charms and purifications.**[83]** Yet if the purifiers were indeed vulnerable to some of this writer's criticisms, the element of bravura in his own typology of mental illnesses, some due to phlegm, some to bile, is surely amazing. It is only those with unbounded faith in our author and already convinced of the correctness of Hippocratic naturalism who would believe that he knows much more about what he is talking about in that context**[84]** than the purifiers who diagnosed one type of epilepsy as the work of Poseidon, another that of Ares, another that of the Mother of the Gods.**[85]** True, his framework of explanation *is* naturalistic, not religious or supernaturalistic. Yet the operations of phlegm and bile to which he appeals, while in principle verifiable, remain at the level of pure speculation. Those operations are invisible entities too, if of a different kind. As for the treatment this writer recommends, it relies largely on the attempt to control the hot, the cold, the wet, and the dry by regimen. Having proclaimed that "the majority [of maladies] may be cured by the very same things from which they arise,"**[86]** he goes on:

[83] See especially *Morb.Sacr.* 1 (L) 6.354.12ff., discussed in G. E. R. Lloyd 1979, pp. 15ff.

[84] On the other hand, the writer's description of the visible signs that accompany an onset of the illness is clearly based on careful observation: *Morb.Sacr.* 7 (L) 6.372.4ff.

[85] *Morb.Sacr.* 1 (L) 6.360.13–362.6.

> A man with the knowledge of how to produce by means of a regimen dryness and moisture, cold and heat in the human body, could cure this disease too [that is, epilepsy, and that is in addition to other diseases, indeed every other disease, madness included],**[87]** provided that he could distinguish the right moment for the application of the remedies. He would not need to resort to purifications and magic and all that kind of charlatanism.**[88]**

As in the attack on the topic of physical diseases, some of the rationalists

are loud in their claims both to superior knowledge and to superior therapeutic efficacy, but in the case of mental illness the bluffing is even more transparent. The establishing of a naturalistic basis for the understanding of madness, the ruling out of references to the divine or demonic, is a release from one mystification. But it was achieved at the cost of the substitution of another of a different kind, at least when the theorists' own proposed explanations were quite unsubstantiated and imaginary. Nor did the positive and constructive help on offer amount to very much. To be told that your madness was not sent by the gods might (if you were convinced) be reassuring. At the same time the convinced rationalists cut themselves off from such support as had been available from traditional social, let alone religious, resources. But otherwise, to have any great expectations of improvement from adopting the anti-bilious or anti-phlegmatic diet of cold, or alternatively warm, food and exercise, prescribed in *On the Sacred Disease* , was clearly, and equally, principally a matter of faith.**[89]**

Dreams

On madness the Hippocratics generally refused to be moved by those who would have celebrated it in one or another of its forms. On the topic of dreams,**[90]** however, many of them were persuaded by, or at least accepted, common Greek beliefs. Among those beliefs was a realisation that not all dreams are significant, not all are veridical: there is the Gate of Horn, but there is also the Gate of Ivory.**[91]** Yet it was widely held that many dreams contain a message, even if not necessarily an obvious or direct one, but one needing interpretation. From the Hippocratics down to Galen and beyond many doctors, including some of the foremost spokesmen for the anti-irrationalist point of view, accept some role for dreams in diagnosis, even if they do not endorse particular practices such as that of temple incubation, the soliciting, as it were, of dreams from the god, including, especially, dreams relating to

[90] Among important recent discussions of aspects of ancient dream theories the following may be noted especially: Dambska 1961, Kessels 1969, 1978, Wijsenbeek-Wijler 1978, Huby 1979, Cambiano 1980a, 1980b, van Lieshout 1980.

[91] *Od.* 19.560ff. Among the notable examples from ancient literature where the question of the fulfilment or otherwise of a dream or prophecy becomes a matter of remark are *Od.* 2.201f., Aeschylus *Agamemnon* 249, *Choephori* 523–34.

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the dreamers' illnesses or containing instructions about their cure.**[92]** Thus *Epidemics* 1.10 includes "dreams, their nature and their time" among the items in the general list of factors to be considered in diagnosis.**[93]** In the Hellenistic period Herophilus even includes "god-sent" dreams in his classification of the three main types.**[94]** Galen not only tells us that his father was guided by a dream in deciding that Galen himself should take up a medical career, but also refers to therapies that he says were suggested to him by dreams.**[95]**

In time,**[96]** as is well known, complex theories were developed not

[92] The classic study of temple incubation remains Deubner 1900. One of the most extensive ancient sources for the practice is Aelius Aristides *Orationes (Or.)* 47–49.

[94] Aetius 5.2.3, discussed in von Staden, forthcoming; cf. Kessels 1969, pp. 414ff. Cf. also the attention paid to dreams in Rufus *Quaestiones medicinales* 5, *CMG Suppl.* 4.34.13ff. For later discussion and elaboration of the idea of "god-sent" dreams, see, for example, Artemidorus *Onirocritica* 1.6.15.19–16.9, who disagrees with the view expressed by Aristotle in the text cited below, n. 103, Iamblichus *De mysteriis (Myst.)* 3.2.103.8ff. It is particularly striking that among the Hellenistic philosophers the aggressively naturalistic Epicureans allow that simulacra may come to humans from the divine, for example in dreams, though such simulacra are to be explained not as portents but as effluences: see Epicurus *Ep.Hdt.* 10.51, cf. Cicero *De natura Deorum (ND)* 1.18.46ff., D.L. 10.32, Lucretius 5.1169ff., cf. 4.757ff. Cf. Clay 1980, Schrijvers 1980.

[95] Galen mentions his father's dream at (K) 10.609.8ff., *CMG* 5.8.1.76.29ff. ([K] 14.608.15ff.), (K) 19.59.9ff. At (K) 11.314.18ff., cf. (K) 16.222.10ff., Galen confides in his reader that he was led to a treatment involving arteriotomy by a dream, and he discusses diagnosis from dreams at some length in his commentary on book 1 of the *Epidemics, CMG* 5.10.1.108.1ff. ([K] 17A.214.7ff.) (cf. what is generally considered the spurious compilation at [K] 6.832.1ff.), *Subfiguratio empirica (Subf.Emp.)* 78.26ff., cf. (K) 12.315.10ff.: see most recently Oberhelman 1983, cf. Demuth 1972, Guidorizzi 1973.

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only distinguishing the various types of dreams—predictive and non-predictive, allegorical and non-allegorical—but also setting out in detail how they should be interpreted. Many of the writers in question, such as Artemidorus,**[97]** are sophisticated, at points quite cautious, restrained,

even self-deprecatory. Many topics of interest might be pursued here. One we may note in passing is the extent to which the importance of wish-fulfilment is recognised by Greek dream-theorists: Freud himself remarked, rather defensively, in the 1914 edition of *Die Traumdeutung* , that those who attach any importance to such anticipations can go back to classical antiquity, and he cited Herophilus in particular in this connection, while he still insisted that no one before him had held that *every* dream is a wish-fulfilment.**[98]** However, our chief concern here must be with the kinds of theories and explanations our early would-be rationalists offered to account for the phenomena.

Once again we have a whole Hippocratic treatise devoted to the subject, the fourth book of *On Regimen* (sometimes called *On Dreams*), as well as an important discussion in Aristotle which (whether or not he knew *On Regimen* 4) develops a similar theory.**[99]** Since Aristotle con-

[98] Freud 1953, vol. 4, p. 132 n. 2.

fines himself largely to providing a general framework of explanation, it will be convenient to reverse the chronological order and take him first.**[100]**

Dreams correspond to movements in the body, notably in the sense organs themselves, these movements being transmitted to the soul.**[101]** During the day many of these movements go unnoticed in the welter of impressions the soul receives. But at night, when the soul is less preoccupied, traces of some of the daytime impressions may be registered in it, provided the soul is itself in a stable condition.**[102]** No dreams are sent by the gods, though Aristotle says that they are

δαιμόνια

, on the grounds that nature herself is

δαιμονία

.**[103]** That makes dreams natural, but serves to remind us that for Aristotle nature is something to be

[100] On the date of *Vict.* views have differed widely, and no precision is possible. Internal evidence shows that the author of *Vict.* 1.4–5 is familiar with the work of Heraclitus, Empedocles, and especially Anaxagoras, but the use made of their ideas is compatible with a fourth- as well as a late fifth-

century date. W. D. Smith's recent attempt (1979) to establish that this is an authentic work of Hippocrates himself has not won wide agreement. The work shows no signs of Hellenistic influence, however, and a date before Aristotle's treatises *De somno et vigilia (Somn. Vig.), De insomniis (Insomn.)* and *De divinatione per somnia (Div.Somn.)* seems likely.

[101] *Insomn.* 459a11ff., 24ff.

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revered.**[104]** Most of what were believed to be prophetic dreams are mere coincidences, but some are signs, some causes, of future events:**[105]** they are causes in that they may suggest a course of action that the dreamer then puts into effect; they are signs in the manner Aristotle has explained, when they provide information about movements and changes in the body—though even here, if the soul is not itself stable, the information will be garbled. It is skill in recognising similarities that makes the best interpreter of dreams.**[106]**

That dreams may thus indicate points relevant to the health of the dreamer is a view that Aristotle ascribes to the "more discerning doctors," and one he endorses himself.**[107]** Whether or not it was written by one of those "discerning doctors," *On Regimen* 4 offers an elaborate working-out of that idea.**[108]** After first setting out a psychological theory according to which (as in Aristotle) the soul is distracted by waking impressions but while asleep "it becomes master in its own house,"**[109]** the writer takes pains to differentiate himself from others in the field:

> As for the god-given dreams which give to cities and to individuals fore-knowledge of bad things and of good, there are interpreters with their own art in these matters. Such people also interpret the signs derived from the soul which indicate bodily affections in advance: excess, whether of repletion or depletion, of what is natural, or some unusual change. In such matters they are sometimes right and sometimes wrong,

[104] Cf. *PA* 645a16–23.

[105] *Div.Somn.* 462b26ff., 463a21ff., 30ff., b29ff.

[106] *Div.Somn.* 464b5ff., 10ff.

[107] *Div.Somn.* 463a4ff.

[109] *Vict.* 4.86 (L) 6.640.2ff., 6ff., cf. Pindar fr. 116, mentioned above, n. 99.

> but in neither case do they know why it happens, neither when they are right nor when they are wrong. But they give advice to beware of taking harm: and yet they do not teach you how you ought to beware, but merely instruct you to pray to the gods. Prayer is a good thing, but one should take on part of the burden oneself and call on the gods only to help.**[110]**

In fact, in the subsequent discussion, prayer is recommended from time to time, and even the gods to whom it should be addressed are specified. "Pray to Earth, Hermes, and the Heroes," for instance, when the dream is of the earth looking black and scorched—which indicates excessive dehydration in the body.**[111]** However, the writer is scrupulously agnostic about whether to go beyond prayer and engage in apotropaic rituals: when dreams are contrary to the acts of the day, this indicates disturbance in the body which may be severe or mild, but "on whether or not you ought to avert the act [that is, by appropriate rites], I pass no judgement."**[112]** The burden of his thesis throughout is that each kind of dream corresponds to a particular physical illness or malfunction which steps can be taken to remedy.

While he dismisses rival interpreters with his curt "they do not know why it happens," no arguments are here offered for his own the-

[111] *Vict.* 4.90 (L) 6.656.22ff., cf. 4.89 (L) 6.652.17ff.

ory, only assertions, although some of the underlying assumptions, and his use of symbolism, are transparent, and traditional, enough.**[113]** Broadly and simplemindedly, to see good things in dreams is good, and bad things bad, and again it is good to see things that correspond to daytime thoughts and actions and that represent them as occurring in an orderly fashion.**[114]** More specifically we are told that it is a sign of health if, in the dream, when a star seems to fall out of its orbit, it appears pure and bright and moves eastwards.**[115]** Conversely, "when [a star] seems dark or dim or to move westwards, or towards the sea, or towards the earth, or upwards, these signify diseases. Upward movements indicate fluxes in the head; movements towards the sea, diseases of the bowels; and those towards the earth, usually tumors growing in the flesh."**[116]**

Moreover, confident recommendations for treatment match confident diagnoses:

> Should one of the stars seem to be injured, or to disappear, or to be obstructed in its orbit, if this happens because one sees it affected by mist or cloud, this is a weak sign, but it is a more severe one if by water or hail. It signifies that an excretion of moisture and phlegm has occurred in the body, and has fallen towards the outermost circuit.**[117]** In such cases it is beneficial for the patient to take long runs, well wrapped up: they should gradually be increased to cause as much sweating as possible. The exercise should be followed by long walks and the patient

[113] As, for example, in the use of black-white symbolism in *Vict.* 4.91–92 (L) 6.658.8, 10, 13, 18.

[114] As, for example, at *Vict.* 4.88 (L) 6.642.11ff., cf. 4.93 (L) 6.660.15f., which suggests that seeing customary things indicates a desire of the soul.

[115] *Vict.* 4.89 (L) 6.650.4ff.

[117] The writer has a theory of three main circuits or orbits of the heavenly bodies, the stars outermost, the sun in the middle, and the moon below that (*Vict.* 4.89 [L] 6.644.18ff.), and he assumes that the main parts of the human body are disposed in three corresponding circuits (cf. *Vict.* 1.10 [L] 6.486.3ff.).

> should go without breakfast. Food should be cut by a third and the normal diet restored gradually over five days. If the disorder appears more severe, prescribe steam baths in addition.**[118]**

Analogies with more modern health faddists are not hard to suggest.

The limitations of this rationalist takeover are twofold. First, the field of what is taken over is restricted. This writer is not concerned (though Aristotle was) with the whole range of predictive dreams, about some of which he is quite indifferent; he concentrates, rather, on what can be discovered about the state of health of patients from their dreams. He has an entirely naturalistic theory of the correlations between the two. No god sends these signs; they are the natural by-products of physical disturbances, a theory he elaborates with some persistence. But it is limited also in a second sense, in that, although superior knowledge is claimed, in practice the theory draws heavily on, and at points is merely a rationalisation of, popular beliefs. Yet the ambition to go one better than traditional views and even than specialist

interpretations is evident from those claims to superior knowledge.**[119]** The specialists are said not to know what they are talking about—whereas the Hippocratic writer, armed with his naturalistic theory of physical-psychical correlations, can, if you believe him, put the whole "science" on a firm footing.

Divination and Fate

The limitations, and pretensions, of the inquiry concerning nature emerge clearly once again in relation to the final pair of topics I mentioned, divination and fate.**[120]** Here one might have expected the proto-scientists to have abstained from confrontation with the likes of Teiresias and Cassandra.**[121]** Even if some dreams might be scrutinised for the diagnostic signs they might yield about the dreamer's *current* state of health, the idea of trying to predict the future, especially the individual's future, was, one might have thought, a palpably unpromising area for any kind of research that purported to involve the inquiry concerning nature, even if it might be the concern of a moral philosophy, whether deterministic or anti-deterministic.**[122]**

[120] Many aspects of divination in the ancient world are well analysed in the collection of essays in J.-P. Vernant et al. 1974: see especially Vernant's own contribution, pp. 9ff., which points out, for example, the solidarity, in many ancient societies, between divination and other forms of rationality, and cf. Bottéro's study of divination in Mesopotamia, pp. 70ff., especially 153ff., 168ff., 190ff., in which he argues, among other things, that curiosity in general may be stimulated by the ambition to foretell the future, and that in that sense divination may be seen as leading to science as well as being itself a science insofar as it has claims to be deductive, analytic, and systematic. Among recent anthropological discussions of divination should be mentioned those of Moore 1957, Park 1963, Jules-Rosette 1978, and Ahern 1981, particularly. Moore and Park in particular provide comparative material to illustrate how divination may sometimes serve to take the question of responsibility for particular decisions out of what is perceived as the human domain or to introduce a randomisation process into such decisions.

[121] Xenophanes, indeed, is reported to have rejected all forms of divination (Cicero *De divinatione (Div.)* 1.3.5, Aetius 5.1.2) but he was clearly exceptional, if not unique, in the pre-Socratic period.

[122] As is well known, the issue of determinism itself once it became, as it did in the post-Aristotelian period, a central topic of philosophical debate was discussed as a physical as well as a moral problem. Both the Stoics, who asserted determinism, and the Epicureans, who denied it, argued their case

in the first instance by reference to natural causation, the Stoics maintaining that there is an inexorable nexus of physical causes and effects and the Epicureans postulating the swerve precisely to constitute an uncaused exception to that rule. It is clear that the Epicureans used this doctrine not just to explain cosmogony but also to insure free will, though quite how what is, *ex hypothesi* , an uncaused movement of soul atoms is to secure the latter remains controver-sial (any such movement in the soul at the moment of choice would appear to tell against personal control of that choice or moral responsibility for the action: the issue is helpfully discussed by Furley 1967; cf. Long 1974, pp. 56ff.; and, most recently, Sedley 1983a and Don Fowler 1983). The notions of insisting on a separation first between physical and psychological determinism, and secondly between the question of the nature of physical laws and the issue of moral responsibility, which have figured prominently in many modern discussions of the problems (e.g., Pears, ed., 1963, Popper 1965/1972, Lucas 1970, Anscombe 1971, O'Connor 1971, Kenny 1975) run counter to the general tendency, in ancient debate, to run together the physical and the moral philosophical questions. Not even the Stoics, who held the doctrines both of moral responsibility and of physical determinism, argued that the latter is irrelevant to the former but, rather, that the former is qualified by the latter. Again, the Epicureans may have assumed that to secure free will it was necessary (if not also enough) to show or, rather, to assert exceptions to the nexus of physical causes and effects. The literature on the post-Aristotelian debate is immense: apart from the studies already mentioned, see especially Long 1971, 1977, Donini 1973, 1974–75, M. Frede 1974, 1980, Sharples 1975a, 1975b, 1981, 1983, Reesor 1978, Stough 1978, Sorabji 1980a, 1980b, Sedley 1980, D. Frede 1982, Annas 1986.

Yet first, the discourse of prediction, which is prominent in our scientific vocabulary when we talk of the predictive value of a hypothesis, encompassed in the ancient world too a wide range of phenomena. In two areas, especially, it is legitimate to talk of ancient *scientific* predictions. Whereas modern medicine is concerned with diagnoses, the ancients often focused, rather, on "prognosis,"**[123]** especially the outcome of the disease, and we need not doubt that, drawing on a wide experience and sometimes despite some simpleminded theories, many Greek doctors were often able to anticipate not just the recovery or death of their patients but also the general progress of their ailments.**[124]** Again,

[123] But "prognosis," for the ancients, concerned the past and the present as well as the future of the disease, as is clear, for example, from *Prog.* 1 (L) 2.110.2f., quoted below, n. 126.

[124] Thus confident and often well-grounded pronouncements on the likely results of certain lesions are common in the surgical treatises: see, for example, *Art.* 63 (L) 4.270.3ff.; 69 (L) 4.288.3ff. We shall be returning later

to the ancient debate on whether or how far medicine can be deemed to be an exact science, but many of those who insisted that it is not maintained nevertheless that it is a rational inquiry that can and does yield knowledge: see below Chap. 3, at nn. 88ff., Chap. 5, at nn. 134ff.

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and closer still to the modern analogue, once Greek theoretical astronomy had become established,**[125]** the models proposed could be, and were, used to predict the positions of the sun, moon, and planets.

Secondly, the overlap between the better and the less well-grounded predictions was recognised by some of the ancient writers, both in medicine and in astronomy. Some Hippocratic treatises recommend the practice of prognosis in terms that are obviously reminiscent of the role of the prophet. When the writer of the work called *Prognosis* says that the doctor should "tell in advance, in the presence of his patients, the present, the past and what is to come to be,"**[126]** or again when we find in *Epidemics* 1.5 the recommendation to "declare the past, determine the present, foretell the future,"**[127]** the echoes of Calchas in *Iliad* 1 or of the Muses in the *Theogony* are obvious.**[128]***Prognosis* advocates forecasting in part so that the doctor will not be blamed for failure: he cannot be held responsible if he has foretold an unfavourable outcome to a case from the beginning.**[129]** But in part the aim is to increase the

[125] Although there are doubts about the extent to which Eudoxus, in the fourth century B.C. , offered exact quantitative models of the planets, sun, and moon, such models were certainly given by Hipparchus in the second century B.C. (if not in the century before him by Apollonius) for the sun and moon, and by Ptolemy in the second century A.D. (if not by Hipparchus) for the planets.

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doctor's reputation. Patients will more readily entrust themselves to his care if he can tell them not just the outcome of the disease, but its past course and their present condition,**[130]** and the doctor will "justly be an object of wonder."**[131]** The surgical treatises, too, though wary of certain types of ostentation,**[132]** occasionally endorse the practice of forecasting, and not just, for example, to warn the patient of the risks of treatment.**[133]** Thus *On Joints* speaks of "brilliant and competitive forecasts" with approval.**[134]**

The writer of *On Regimen in Acute Disease* , however, sees the danger of

medicine being confused with divination. The reputation of medicine is harmed by disagreements among doctors, which undermine the art to the point where some might even say that it resembles divination or the inspection of entrails and that doctors are like seers quarrelling about the interpretation of omens from birds.**[135]** *Prorrhetic* 2 opens with an attack on the idea that "marvellous" and exact predictions are possible in medicine.**[136]** The author says that he will not

[132] See below, Chap. 2, pp. 69 and 99.

[135] *Acut.* 3 (L) 2.242.3ff. The difficulties of making predictions in acute diseases, for example, are mentioned at *Aph.* 2.19 (L) 4.474.12f.

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engage in such "divinations" but will set out reliable signs by which one can recognise which patients will recover and which die.**[137]** Other predictions should be made

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—in a more modest fashion, as befits mere human beings**[138]** —though he too will set out what you have to know if you want to succeed in this kind of competition.**[139]** As for the exactness sometimes claimed for medical forecasts, he says that he listens and he laughs.**[140]**

The doctors thus evidently used their ability to foretell the future and to retrodict the past as a means of impressing their patients and indeed of building up their practice. Yet there was a risk of the doctor being assimilated to the soothsayer, a risk some Hippocratic writers try to guard against, and which much later Galen repeatedly tells us he still had to contend with.**[141]** Some of the medical men actively sought a reputation for being able to predict the future,**[142]** even while they dis-

[138] *Prorrh.* 2.2 (L) 9.8.11ff., cf. 2.3 (L) 9.10.23ff.

[140] *Prorrh.* 2.4 (L) 9.14.10f., cf. 20.11ff. The difficulty of making predictions before the disease has become established is mentioned at 2.3 (L) 9.12.20ff., 14.2ff.

[141] Galen tells us that his use of the pulse in diagnosis was considered to be mere divination by his critics, e.g., *CMG* 5.8.1.106.21ff. ([K] 14.637.10ff.) and elsewhere reports that he was suspected of magic, charlatanry, and divination (see [K] 7.354.4ff., 11.299.10ff., 301.10ff.,

12.263.6ff., *CMG* 5.8.1.84.5ff., 94.18f. [(K) 14.615.4ff., 625.16f.]), although he himself accuses others in similar vein (e.g., [K] 11.793.12ff., 795.16f., 796.7ff.). It is quite clear that on occasion he deliberately sought to amaze his audience (e.g., [K] 8.361.12ff., 365.9ff.), especially in the context of spectacular exhibition dissections (e.g., *AA* 8.4 [K] 2.669.7f., 15) while he criticises others for doing the same (e.g., [K] 11.797.10ff.), cf. Kollesch 1965, Vegetti 1981.

[142] Indeed, after the classical period medical divination became systematically linked with astrology in the iatro-mathematical tradition: see, for example, Ptolemy *Tetrabiblos (Tetr.)* 1.3.16.7ff.; 3.13.147.9ff.; 4.9.200.12ff. Boll-Boer; cf. Bouché-Leclercq 1899, chap. 15, pp. 517–42.

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sociated themselves from others who, in other contexts, had precisely the same ambition.**[143]**

The ambivalence of the relationship between—to use our terms—astronomy and astrology is more highly charged still, and many have simply dismissed the latter as an aberration. The fact that most prominent ancient astronomers, including Hipparchus and Ptolemy, also engaged in astrology is often taken to be irrelevant to Greek *science* and as evidence only of the failure of the Greeks to be *scientific* . Yet not to be guilty of gross anachronism, we must take as our explananda not just those parts of ancient mathematics and natural philosophy that we approve or consider fruitful, but the whole of the corpus of work of those who engaged in different branches of those complex and manifold traditions. To ignore astrology would be to miss the insights it can offer both about ancient controversies concerning what those traditions comprised and about the ambitions some theorists entertained concerning some areas that they were certainly eager to include.

That some parts of their work were better grounded than others goes without saying; it went without saying to the ancients themselves, even while they, like us, argued about the criteria of superiority. Ptolemy, for one, clearly distinguishes between the two types of prediction or prognostication to be made from the study of the heavenly bodies: on the one hand, predictions of their movements (astronomy in our sense); and on the other, prediction concerning events on earth.**[144]** Moreover, he explicitly emphasises the conjectural nature and the difficulty of the latter study,**[145]** criticising the excessive claims made

[144] See, for example, Ptolemy *Tetr.* 1.1.2.16ff.

[145] *Tetr.* 1.1.3.5ff., 2.8.1ff.; 3.2.109.1ff.

by some past and contemporary practitioners**[146]** and limiting his own discussion to generalisations based on the supposed beneficence or maleficence of various heavenly bodies or their configurations.**[147]** The validity of astrology as a whole was disputed,**[148]** but we should remember that there were similar foundational disputes in many other areas of the inquiry concerning nature, including about astronomical model-building itself.**[149]** Based on a belief in a connection,

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,**[150]** between heaven and earth, which could be illustrated, in the first instance, by such uncontroversial examples as the seasons and the tides,**[151]** astrology was usually defended (like medicine) primarily by reference to what were claimed as its results,**[152]** and as in medicine again, there was considerable indeterminacy in evaluating these.

[146] *Tetr.* 1.2.7.20ff., 22, 52.11ff., and especially 3.4.113.18ff., where Ptolemy dismisses aspects of genethlialogy as currently practised as superfluous nonsense and as lacking any plausibility.

[147] *Tetr.* 1 and 2 deal with general astrology, 3 and 4 with genethlialogy.

[148] Cicero's *De divinatione* provides the most comprehensive ancient account of the arguments used *pro* and *contra* the possibility of divination in general.

[149] Thus although Proclus gives a detailed introductory account of astronomy in his *Hypotyposis astronomicarum positionum* (*Hyp* .), this is prefaced by a passage (pr. 1.2.1ff.) where he cites Plato for the view that the true study concerns the region "beyond" the heavens and is directed to the immutable Forms. Moreover, he repeatedly expresses his view, both in *Hyp.* and in his *Commentaries* to the *Republic* and *Timaeus* , that the epicycle-eccentric model is complicated, farfetched, and arbitrary (see, for example, *In Platonis Timaeum commentarii (In Ti.)* 3.56.28ff., 146.14ff., *Hyp.* 7.236.10–238.21); cf. Sambursky 1965. From the life sciences we shall be considering later the dispute over the validity of the practice of animal dissection: Chap. 3 at nn. 220ff.

[150] The idea was used by the Stoics especially (see, e.g., Cicero, *ND* 2.7.19, *Div.* 2.14.33ff., Sextus *Adversus mathematicos* [*M* .] 9.75ff., 79ff., cf. 5.4ff., Cleomedes *De motu circulari corporum caelestium* 1.1.4.10ff., 8.15ff., Alexander *De mixtione* [*Mixt* .] 3.216.14ff., 11.226.30ff., 12.227.5ff.) but was not confined to them: see, for example, Philoponus *In*

Aristotelis libros de Generatione et Corruptione commentaria (*In GC*) 41.25f., *In Aristotelis Physica commentaria* (*In Ph.*) 113.8f., and for its use in Soranus' gynaecology, for instance, see G. E. R. Lloyd 1983a, pp. 178ff.

[151] See Ptolemy *Tetr.* 1.2.4.3ff. The way Ptolemy moves from these incontestable cases to far more dubious ones has been analysed especially by Long 1982.

[152] Thus Ptolemy repeatedly refers to what he says the ancients had observed to be the properties or qualities of the heavenly bodies, e.g., *Tetr.* 1.3.17.7ff., 9.22.21ff., 10.30.6f., cf. 12.32.23ff., though he is aware of competing traditions on some points, e.g., 1.21–22, 44.22–53.13. Analogies with medicine, navigation, and archery are invoked, e.g., at 1.2.10.2ff., 3.13.16ff.; 3.2.109.11ff., to distinguish, for example, between the errors of individual practitioners and the general soundness of the art.

The case for it seemed untenable to many,**[153]** but others exploited what seemed to them a splendid opportunity to bring this area of divination too into the orbit of rigorous mathematical disciplines. Even though astrology began in Greece with vague ideas about the influences of the stars such as we find in Hesiod,**[154]** it came to have, in Hellenistic times, an elaborate theoretical framework most of which it owed to astronomy.**[155]** In this, astrology was importantly different both from divination by the consultation of books, for instance (as in the sortes Homericae or Virgilianae), which involved no study of natural objects at all, and from hepatoscopy, for there the marks on the surface of the liver that the diviner studies are, as Rufus gives us to understand, of no significance for the medical man.**[156]** The use of planetary tables and of spherical geometry is *common* to astronomy and astrology, both of which engage in the determinations of planetary positions. Initially just impressionistic, astrology came to have claims to be, in some respects at least, an exact study. Certain assumptions (and they were of course the crucial factor) had to be made about what were claimed as the natural effects of different heavenly bodies or at least about how they could be used as signs; and the application of general rules to individual cases was always a matter of the astrologer's own judgement. Yet planetary configurations could be worked out with impeccable mathematical precision and deductive rigour. In that respect

[153] At *Tetr.* 1.1.3.15ff., before Ptolemy sets out to defend astrology against the specious arguments brought against it, he remarks that what is hard to attain is easily attacked by "the many."

[154] See, for example, *Op.* 417ff., 587f. (cf. 609ff.) on the effects brought about by, or associated with, the star Sirius.

[155] As has been remarked, for example, by Neugebauer 1952/1957, p. 171; cf. 1975, vol. 2, pp. 607ff.

the casting of horoscopes or genethlialogy could claim to be more exact than most areas of medicine or of natural philosophy. The symbiosis of the two studies of the heavenly bodies is remarkable, for on the one hand the aspirations of astrology helped to keep elementary astronomy alive, and on the other the prestige of astrology depended partly on its incorporating the same mathematical procedures used in astronomy. It was thanks, in part, to his mathematics that the "mathematician," as he was often known, won his reputation as a superior diviner.

Finally, the importance of astrology, for Ptolemy at least, was underpinned by its ultimately moral aim of helping us view the future with calm and steadiness.**[157]** But this moral concern is in no way exceptional in the inquiry concerning nature in the ancient world. On the contrary, as we shall see in due course,**[158]** it is a recurrent one. For now, we may simply note that, following Plato, Ptolemy held that astronomy too is good for the character,**[159]** and that following Aristotle, Galen claimed that the study of the parts of animals reveals the wonders, beauty, and goodness of creation and instils in the student true piety towards its wise and benevolent creator.**[160]**

[158] See below, Chap. 6, pp. 319 and 336.

[159] At *Ti.* 47b–e Plato suggested that by beholding the revolutions of reason in the heavens we can stabilise the revolutions of our own thinking. Ptolemy claimed that astronomy can help to make men good: *Syntaxis* 1.1, Heiberg (H) 1.7.17ff., quoted below, Chap. 6 n. 144.

Our later investigations will give us ample opportunity to consider aspects of Greek science where traditional beliefs and attitudes are less prominent in the background or even—as often with Archimedes, for instance—quite irrelevant. This first study has been deliberately directed at a set of highly problematic and difficult topics for science, where myth, religion, and ritual provided the usual resources for stabilising belief. Those who engaged in the inquiry into nature—those who invented that inquiry—exhibit some well-grounded confidence in their ability to provide an alternative, naturalistic, rationalist framework for understanding. At the same time they often, it may

seem to us, fail to recognise the limitations of what they had achieved or of what they could hope to achieve, both where the questions they raised are simply not amenable to their approach (certainly not in their day, and in some cases not even now) and where the answers they proposed are vulnerable, if in different ways, to criticisms similar to those they themselves brought against earlier beliefs.

Yet that recurrent phenomenon may be understandable in part at least in terms of the problems the new investigation into nature faced in establishing itself alongside and in confrontation with other more traditional sources of wisdom, comfort, and understanding. Some of the investigators themselves claimed that theirs was the way not just to understand nature, but to gain a correct apprehension of the divine:**[161]**

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true piety consisted in the type of study in which they were themselves engaged.**[162]** Others did not so much transmute as undermine traditional systems of belief, attacking in particular some of the authority figures who sustained them—the prophets, diviners, purifiers, and the like.**[163]**

[163] There were, of course, as we have already noted (n. 161), others besides those who were directly and principally engaged in the study of nature who criticised conventional religious beliefs and practices. The most radical attack came from philosophers and sophists who rationalised the origin of such beliefs. Thus Prodicus is reported to have suggested that men treated as gods things from which particular benefit was derived, such as bread and wine, as well as water, fire and the sun, moon and rivers (see, e.g., Sextus *M.*9.18 and 52, cf. Henrichs 1975). But a recurrent motif in such rationalisations is an association of the divine with terrifying natural phenomena. In the famous fragment from Critias' *Sisyphus* (fr. 25) that suggests that the gods were a deliberate human invention introduced to provide a sanction to insure good behaviour, the imagined human inventor locates the gods in the upper circuit from which lightning and thunder come to frighten mankind. Although Democritus did not dismiss notions of the gods entirely, he too is reported by Sextus (*M.* 9.24) to have argued that belief in the gods is in part a mistaken inference from terrifying natural phenomena such as thunder, lightning, and eclipses, and the atheists criticised by Plato in the *Laws (Lg.* 889e ff.) are represented as claiming that the gods exist "by art" and "by convention" rather than by nature. But whether or not it was the *intention* of any natural philosopher who attempted purely physical explanations of thunder, lightning, eclipses, and so on to substitute a naturalistic account for a religious or super-naturalistic one, the *effect* of the new search for aetiologies was to make available what could be seen as adequate alternative frameworks for understanding. "What Zeus?" as Socrates is made to ask in Aristophanes' *Clouds (Nubes, Nu.)* 367ff., "there is no Zeus." And to the question of Strepsiades, "who then rains?" Socrates replies that it never rains without clouds.

Moreover, the explicit *intention* to remove any involvement of the gods in such natural phenomena is clear in Epicurus, for whom the primary motivation of the study of nature is, precisely, to rid men of such superstitious beliefs: *Ep.Hdt.* 10.76ff., *Letter to Pythocles* (*Ep.Pyth.*) 10.85ff., 97, 113, *Sent.* 11, 12.

But, implicitly or explicitly, the investigators into nature laid claim to a new kind of wisdom, a wisdom that purported to yield superior enlightenment, even superior practical effectiveness, and in part (though this can only be justified by the detailed studies that follow) they were surely right in their conviction of their own originality and their belief in the potentiality of the approach they adopted, even though their strengths had their corresponding weaknesses, especially in the excesses and exaggerations of many of those claims. They were wise men of a different kind, unlike the old seers in important respects, though again much closer to them in others than aspects of the self-image they projected would lead one to expect. They successfully demystified many a mythical, mystical, symbolic, or traditional assumption. For all that, the science they presented was, in some cases, no more than the myth of the elite that produced it. These are themes that will be explored more fully in the remainder of this book.

Chapter Two— Tradition and Innovation, Text and Context

"Tradition" and "innovation," according to a view once expressed by Thomas Kuhn,**[1]** are the two elements in scientific research between which there is, or should be, what he called an essential tension. At one extreme, we may say, tradition with no innovation at all equals stagnation. At the other, innovation with no tradition at all would produce unintelligibility. While innovation and flexibility have been prominent features of the self-image of science, Kuhn chose to stress the opposite factor, that is to say, the need for tradition and the positive role of dogma in research, putting it that it is precisely the abandonment of critical discourse that marks the transition to science.**[2]** Some aspects of the dogmatism of Greek science will be discussed in due course in Chapter 3. My aim in the present chapter is to explore some of the modalities of tradition and innova-

[1] Kuhn 1959/1977 and 1963.

[2] Taking issue with Popper's discussion of pre-Socratic thought especially (Popper 1958–59/1963, pp. 136ff.) Kuhn 1970/1977, p. 273, wrote: "It is the tradition of claims, counter-claims, and debates over fundamentals which, except perhaps during the Middle Ages, have characterized philosophy and much of social science ever since. Already by the Hellenistic period mathematics, astronomy, statics and the geometric parts of optics had abandoned this mode of discourse in favour of puzzle solving. Other sciences, in increasing numbers, have undergone the same transition since. In a sense . . . it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy."

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tion at the very beginning of the Greek inquiry into nature, where, as so often, the problems are especially acute, and not just because of the usual difficulties set by the fragmentary and partial nature of our sources. What kinds of innovation, what type of tradition, were needed for that inquiry to gain its initial purchase? And what—if we may at least raise the major question, fully aware that no satisfactory answer may be forthcoming—what stimulated, or what permitted, such innovativeness in the first place?

Comparative Perspectives

Innovativeness, let it be emphasised at the outset, is, of course, no prerogative of the ancient Greeks.**[3]** Rather, it is manifested to a greater or lesser degree by *every* human society. This is not to deny the enormously conservative tendencies in what are often not inappropriately called "traditional" societies. Yet first, a measure of adaptability is, no doubt, a necessary condition for survival. Secondly, the rich store of detailed informal knowledge, for example of plant kinds and their uses, of animals and of animal behaviour—the whole of what Lévi-Strauss discussed under the rubric of "concrete science"**[4]** —must be seen as gradually accumulated over extended periods of time. So too were the technological advances that brought about what Gordon Childe called the "urban revolution" in the ancient Near East.**[5]** All of this presupposes sustained inquisitiveness and intellectual acquisitiveness, even though the additions made to the store of knowledge may well not be identified, let alone recorded, as such. Where the ancient Greeks eventually developed a minor genre of literature devoted to the "first discoverers,"

πρῶτοι εὑρεταί

, of arts, techniques, objects, and ideas,**[6]** the inventions that are recognized as such in concrete science are more likely to be ascribed to mythical or legendary gods or heroes

[3] Cf., e.g., Goody 1977, especially chaps. 2–3.

[4] Lévi-Strauss 1962/1966.

[5] See, for example, Childe 1936/1956, 1942, 1958.

[6] Kleingünther 1933 remains the classic monograph on this topic.

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than to figures securely framed as what we should call historical personages—though we should note that the Greek "first discoverers" also include plenty of the former as well as of the latter: Prometheus and his like, as well as Archytas, Archimedes, and theirs.**[7]**

Even though, as I noted in Chapter 1, tradition is what is usually appealed to both to justify certain ways of behaviour and to block certain types of question, what is included in concrete science or as common knowledge is, nevertheless, subject to adjustment—to the tinkering characteristic of the *bricoleur* ,**[8]** if not to planned or self-conscious revision. Even the actors' own protestations of the sacrosanctity of the tradition do not preclude the possibility of aspects of that tradition undergoing modification and scrutiny, even if not necessarily formal or institutionalised scrutiny. In two areas especially, recent anthropological work has thrown light on the degree to which innovations are possible, occur, and are even inevitable within what is still conceived as an unaltered tradition. First, such studies of oral literature as Goody's on the myth of the Bagre,**[9]** Phillips' on Sijobang,**[10]** and Finnegan's general analysis**[11]** have demonstrated the tolerance of variation in what is still thought of, and unreservedly claimed to be, precisely the *same* nar-

[7] Thus Diogenes Laertius, who refers repeatedly to the topic of first discoverers, offers a very heterogeneous collection of examples, both in terms of the techniques or ideas claimed as invented and in terms of the inventors he names. They range from the attribution of the first predictions of solar eclipses to Thales (1.23) and of the first map of the earth and sea to Anaximander (2.2) to that of the first purifications of houses and fields to Epimenides (1.112). In some cases what is represented by Diogenes as a Greek invention is, rather, a matter of the introduction into Greece of ideas or devices found elsewhere, as, for example, with the gnomon ascribed to Anaximander (2.1) which Herodotus (2.109) says was brought to Greece from Babylonia. Diodorus Siculus, who records Egyptian claims that certain inventions came from their gods, 1.14.1, 15.8, 43.5f., refers explicitly to disputes between one race and another as to who were the first discoverers of things useful for life, 1.9.3, cf. 2.38.1ff., 4.1.6f., 2.5.

[8] Cf Lévi-Strauss 1962/1966.

[9] See Goody 1972, 1977 chap. 3, and forthcoming.

[10] Phillips 1981.

[11] Finnegan 1977.

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rative.**[12]** Secondly, using fieldwork in Thailand, Tambiah has recently investigated the outer limits within which rituals may be altered and adapted to new politico-religious ends and still be apprehended as unchanged.**[13]**

The inference to past innovation is even more compelling where its intellectual products are more obviously exceptional, as is the case with some of those of ancient societies, for example, the mathematics and astronomy of Babylonian civilisation or the mathematics and medicine of ancient Egypt. Here our evidence is written, though some of the extant didactic mathematical texts give glimpses of the oral situation of instruction within which they were presumably used. Thus in the Rhind mathematical papyrus from Egypt we read: "If the scribe says to thee '10 has become 2/3rds and 1/10th of what?' let him hear . . . " and there then follow the workings and the conclusion.**[14]**

Admittedly, we are usually in no position to chart the stages of the developments that occurred, let alone to identify individuals responsible for particular features of them. Nor should we Whiggishly assume that there was a single continuous development, a sustained onward-and-upward march, as opposed to periods of advance interspersed with others of retrenchment, stagnation, and regression. But even though we are in the dark about most of the circumstances surrounding the *growth* of mathematics, astronomy, and medicine in the ancient Near East, the documentary evidence we possess for the *end*

[12] That in oral literature poems are composed not for, but in, performance, and that a given poem is thus recomposed, in a sense, at each performance, were points stressed already by A. B. Lord 1960, pp. 13ff., cf. Nagy 1983. Compare also Detienne 1977/1979, pp. 6, 16, and 1981/1986, pp. 22ff., 37ff., 124ff.

[13] Tambiah 1977, cf. 1982. Compare also Goody 1977, pp. 29f., and forthcoming. In ancient Mesopotamia major myths evidently underwent certain transformations in part in response to changes in the dominant

political power, the most notable example being, perhaps, the substitution of one supreme god—Enlil, Marduk, or Assur—for another in the creation myth Enuma Elish: see, for example, Jacobsen 1949, pp. 182ff., and 1976.

[14] See Peet 1923, pp. 65ff., on number 30; cf. p. 87 on number 47, p. 104 on numbers 61b and 62, and p. 111 on number 68.

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results is substantial, and those end results themselves certainly imply innovation at *some* stage. It is worth laying some stress on that feature of those great civilisations, since it tends to be brushed aside when attention is focused on their undoubted elements of conservativeness and of deference to traditional authority, both in the sense of deference to the customary authority figures and in that of deference to the past.

Those elements, too, are, to be sure, very prominent and they can be exemplified in many different aspects of Egyptian and Babylonian culture, ranging from the social and political sphere to medicine and astronomy themselves. True, our Greek sources typically exaggerate the contrast between, for instance, Greek and Egyptian attitudes. Herodotus,**[15]** Plato,**[16]** and Aristotle**[17]** are among those in the classical period who remark on the conservative tendencies in Egyptian culture, on which Plato, especially, often commented with approval.**[18]** Later, Dio-

[15] Herodotus frequently refers to the care the Egyptians devoted to the preservation of the memory of the past, including their keeping of written records (e.g., 2.77, 100, 145), as well as to their adherence to traditional customs (2.79).

[17] In his discussion of whether it is better to be governed by the best laws or by the best men, Aristotle cites Egyptian medicine, evidently as an example of the former (*Politics, Pol.* , 1286a7ff., 12ff.), though he notes that Egyptian doctors are allowed to alter their treatment after four days; if they do so before, it is at their own risk.

[18] As in the text cited above, n. 16. Sparta and Crete are favourably contrasted with other Greek states for being less inclined to artistic innovation, and in that respect closer to the Egyptian model, at *Lg.* 660b. According to Plutarch *Instituta Laconica* 238c, the Spartans even attempted to forbid innovation.

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dorus**[19]** reported that Egyptian doctors ran a risk of legal sanctions—the death penalty, no less—if they deviated from what the sacred medical books laid down. Moreover, in the case of medicine we do not, of course, have to rely simply on what the Greeks tell us: the extant Egyptian medical texts themselves exhibit the enormous strength and authority of tradition. We have a striking example of this in the Edwin Smith papyrus, a text which preserves medical lore handed down over an extended period, where the glosses contained in the version we have represent the attempts by the final redactors to explain difficult points in the diagnoses of the cases and the treatments prescribed.**[20]** Clearly the chief aim of later physicians was the conservation and faithful interpretation of the wisdom of their predecessors.**[21]**

[20] The Edwin Smith papyrus itself dates from around 1600 B.C. , but the material it contains comes from a much earlier period. Each of the case-histories consists of five sections: Title, Examination, Diagnosis, Treatment, and Glosses. While the first four were composed, according to Breasted 1930, pp. 9f., sometime during the Old Kingdom, between 3000 and 2500 B.C. , the glosses were added at the end of that period: "In the latter part of the Old Kingdom, probably not later than 2500 B.C. , some 'modern' surgeon, as unknown to us as the original author, equipped the document with a commentary in the form of brief definitions and explanations, which we term glosses, appended to each case. For example when the original treatise directed the practitioner to 'moor him [the patient] at his mooring stakes,' the commentator knows that this curious idiom is no longer intelligible and appends the explanation, 'It means put him on his accustomed diet and do not administer to him any medicine.'" Cf. also Breasted 1930, pp. 61ff.

[21] As Wildung, for example, notes in his discussion of Imhotep and Amenhotep (1977, p. 298), in Egypt wisdom was often represented as a matter of learning, the wise man being the man who had direct access to the sources of knowledge, namely, books. On occasion, however, some dissatisfaction appears to be expressed at the constraints imposed by the authority of written texts. This seems to be the gist of a passage in the "Complaints of Khakheperre-Sonb"—to be dated, according to Gardiner 1909, pp. 96f., to the reign of Sesotris II (1897–1878 B.C.), though our extant text belongs to the mid-eighteenth dynasty—where we read: "Had I unknown phrases, / sayings that are strange, / novel, untried words, / free of repetition; / not transmitted sayings, / spoken by the ancestors! / . . . Ancestor's words are nothing to boast of, / They are found by those who come after." (Lichtheim 1973, pp. 145ff.)

Even so the point should not be *over* -emphasised. We may turn to another Greek text to suggest another side to the picture, a report in Herodotus which, despite its patent inaccuracy at certain points, nevertheless has a

lesson to teach us. Herodotus says that, lacking doctors, the Babylonians take their sick down to the marketplace so that any passerby may comment and say how he or anyone else he knew recovered from a similar illness.**[22]** Now it is just not true that the Babylonians had no doctors—though those who acted as such may have looked to Herodotus less distinctive than Greek or Egyptian physicians.**[23]** Yet whatever the flaws in Herodotus' story as a historical report, it serves to remind us that even in generally conservative societies there may be, and often is, a certain open-mindedness about therapy, a readiness to canvass diagnostic opinion and to listen to suggestions concerning treatment.**[24]**

Greek Innovations and Egotism

I have been at some pains to suggest that a certain innovativeness may, indeed must, be supposed to have been at work in the ancient Near East—including in fields that are directly relevant to the understanding of what we call natural phenomena—*even though* we are usually in no position either to date the innovations or to identify their authors. But the significance of that point, in turn, should not be missed. Even though, obviously, innovativeness is no Greek prerogative,

[22] Herodotus 1.197.

[23] The extant Mesopotamian medical texts indicate that there were several different complementary, if not rival, approaches to diagnosis and treatment. The main features are summarised by Oppenheim 1962; cf. 1964, pp. 289ff.

[24] The point can be illustrated extensively from medical anthropology: see, for example, G. Lewis 1975, pp. 248ff.

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that does not mean that the manifestations of innovation are everywhere the same. The extant remains of Egyptian and Babylonian medicine, mathematics, and astronomy can be combed in vain for a single example of a text where an individual author explicitly distances himself from, and criticises, the received tradition in order to claim originality for himself;**[25]** whereas our Greek sources repeatedly do just that. Even where we can *infer* innovations in Egyptian or Babylonian texts, that is to say, it is not the *style* of their authors to publicise the fact or even to mention it.**[26]**

The contrast with Greek writers is striking, and we are dealing, of

[25] Such culture heroes as Imhotep (see Wildung 1977), renowned for the benefits he conferred on mankind, testify to a general Egyptian appreciation of certain types of inventiveness. Moreover, my colleague John Ray draws my attention to two specific pieces of evidence that indicate recognition of particular innovations. The first is a Ramses II graffito that celebrates the achievement of Zoser, more than 1,300 years earlier, who had been the first king to make extensive use of stone in monumental building (see Yoyotte 1960, pp. 57f.). Secondly, the Amenemhet inscription provides an example of a claim for originality in technology, the construction of an improved clepsydra. Amenemhet was active in the reign of Amenhotep I (1545–1525 B.C.). In the inscription published by Borchardt 1920, Amenemhet proudly points out that his clepsydra tells the time accurately throughout the year—that is, that it can register differences between the hours from one season to another (cf. Neugebauer and Parker 1960, p. 119). Two points are, however, to be noted. First, he prefaces his account of the instrument by referring to his study of "all the books of the words of God" (as Borchardt observes, 1920, p. 62, it is not evident whether the knowledge that Amenemhet says he discovered was what he discovered by reading the literature or for himself). Secondly, while he claims that his instrument is better than all others, he expresses this point not in the form that no such instrument had ever been made before him, but in the form that no such instrument had been made "since antiquity." Nevertheless, this inscription is an important—if exceptional—example of a claim to do better than one's immediate contemporaries.

[26] As Grapow has pointed out, 1954–73, 2, pp. 25f., the use of the first person singular in Egyptian medical writings is a characteristic of the magical texts (for example, where the god announces his presence or is invoked as aid, "I am Horus," "I am Theuth," "I have come from Heliopolis . . . assuredly, I have come from Sais with the mother of the gods," cf. Ebbell 1937, p. 29), rather than of the descriptive accounts of the surgical or medical case-histories incorporating observations of particular patients and setting out diagnoses and recipes, though "I" may be used in the verdict on whether the disease is to be treated, e.g., Ebbell 1937, pp. 120ff.

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course, with a phenomenon that extends well beyond the domain of the inquiry concerning nature. Homer's authorial personality remains discreetly veiled—though even Homer has Telemachus remark that it is the newest songs that men praise most.**[27]** But Hesiod writes himself into his poems and states his credentials directly. He has learnt from the Muses, who know how to tell the truth—as well as lies that are like the truth.**[28]** He has, moreover, been victorious, he says, in competition with fellow poets.**[29]** Though certain reservations should, no doubt, be entered in connection with Snell's theses concerning the "discovery" of the mind and the "rise of the individual,"**[30]** he successfully collected, under the latter rubric, a wealth of material relevant to the emergence of the Greek authorial *ego* , notably material from early lyric, from that set of strongly individuated and

aggressively innovative poets that includes Archilochus, Sappho, Alcaeus, and their successors.**[31]**

[28] Hesiod *Th.* 22ff.; on which see, for example, Detienne 1967, pp. 17ff.; Pucci 1977, pp. 9ff.

[30] Snell 1948/1953, chap. 3; cf., e.g., Kranz 1961. There are notable texts in Pindar especially that manifest a certain ambivalence towards innovativeness. On the one hand, he prides himself on his innovations, e.g., *Olympians (O.)* 3.4ff., 9.47ff. On the other, at *Nemeans* 8.20ff. especially he stresses the dangers of bringing what is new to the test and the envy that this is likely to incite (see, e.g., Duchemin 1955, Lefkowitz 1963). While, later, in comedy Aristophanes expresses conservative views on many topics and criticises some of Euripides' innovations in tragedy, for example, he also boasts of the novelty and inventiveness of his own plays, particularly at *Wasps (Vespae)* 1051ff., *Nu.* 545ff.

[31] The increasing authorial presence in various literary genres can, of course, be paralleled by developments in the visual arts and in music. In pot-tery, for instance, the practice of the artist signing his work begins in the seventh century B.C. and reaches a peak at the end of the sixth: see Cook 1972, pp. 254ff. Innovativeness in musical practice, strongly criticised by Plato, was a reality from the fifth century onwards. One of those responsible was Timotheus of Miletus, who is quoted by Athenaeus at 122c–d: "I do not sing ancient songs, for my new ones are better: a young Zeus now is king; Cronos ruled in olden times: away with the ancient Muse." Cf. Aristotle *Pol.* 1341a28ff., who refers to the spirit of experimentation in music, associating it especially with the confidence of the Greeks after the Persian wars, and who also remarks on the role of musical contests, 1341a9ff. There is now an excellent survey of the literary and other evidence in Barker 1984: see especially chap. 7 ("The Musical Revolution of the Later Fifth Century") and the analysis of the evidence in pseudo-Plutarch *De Musica* chap. 15.

Continuing and extending that tendency,**[32]** the natural philosophers and medical writers emerge, indeed thrust themselves forward, as strong and distinct personalities. Even when Xenophanes tells us that no man can know for sure about the gods and the other things he discusses, he makes us aware that it is he who is talking:

ἅσσα λέγω περὶ

πάντων

(fr. 34). Heraclitus appears in the first person to a more marked degree. "Those things for which there is sight, hearing, learning, these are what I,

ἐγώ

, prefer,"**[33]** and in fr. 101 he sums up his "method" with the famous dictum "I sought myself":

ἐδιζησάμην

ἐμεωυτόν

. The point remains valid even when he says that "it is not me but the *logos* that [men] should listen to,"**[34]** for while the *logos* is good for all, common or universal,

κοινόν, ξυνόν

,**[35]** Heraclitus' presence as its spokesman is underlined by, for example, the strong

ἐγώ

in fr. 1:

ὁκοίων ἐγὼ διηγεῦμαι

, "on the matters I explain." Parmenides and Empedocles both refer, in terms that will be understood in part as traditional, to the goddess or Muse who inspires them,**[36]** even though the

[32] A similar tendency manifests itself also, of course, in the historians, where, for example, not just Thucydides and Herodotus but already Hecataeus introduce their works by referring to themselves directly with their proper names.

[33] Heraclitus fr. 55.

[34] Heraclitus fr. 50.

[35] Heraclitus frr. 2 and 113, cf. also frr. 80, 89, 114, 116.

[36] Parmenides fr. 1.22ff., Empedocles fr. 3.3ff., fr. 23.9ff. (First-person statements are common in the extant remains of Empedocles, even though it is often not certain *who* exactly is speaking, the Muse or Empedocles himself:see, for example, frr. 8.1, 9.5, 17.1 and 16, 35.1, 38, 111.2, 112.4, 113.2, 114.1ff.)

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goddess's instruction to Parmenides takes a form that marks a far greater distancing from that tradition, for she tells him to "judge by *logos* the much-contested refutation pronounced by me."**[37]** It is, we should say, the strength of the deductive argument, not the appeal to divine authority, on which Parmenides depends to secure conviction in his hearers, even though he equates the latter with the former. In Ionian speculative natural philosophy, too, Diogenes of Apollonia begins his work by telling us about his method and again leaves us in no doubt that it is *his* : "it seems to me that every *logos* should begin from an incontestable starting-point."**[38]**

Egotism, to be sure, is not necessarily connected with innovativeness,**[39]** but the two often go together in early Greek philosophy, especially in claims to set forth the truth that had eluded everyone else. One after another, the major pre-Socratic philosophers from Xenophanes onwards state or imply that no one else had got the answers right, establishing their own presence in the text with copious criticisms of other writers, their predecessors or contemporaries, named or unnamed, at the limit by criticisms of what everybody else believed. Xenophanes attacks Homer and Hesiod by name for "ascribing to the gods everything that is shameful and a blame among men, thieving, adultery, and deceiving each other."**[40]** Heraclitus, in turn, hammers

[39] As many examples from the Greek orators could be used to illustrate: see, for instance, Antiphon 1.1, 5, 11f.; 6.15f., Lysias 1.5, 22; 3.4, 14; 12.3, 37, among many texts where an orator stresses a personal view with the first-person-singular pronoun or—in ways that are analogous to themes common in natural philosophy and medicine—sets out what he claims to be able to demonstrate.

[40] Xenophanes fr. 11, cf. fr. 12 and the more general attack on anthropomorphic religious beliefs, frr. 14–16 (cf. below, Chap. 4, pp. 176ff.).

Xenophanes along with Hesiod, Pythagoras, and Hecataeus for "much

learning,"

πολυμαθίη

,**[41]** and in many richly abusive fragments expresses his contempt for the ignorance and folly of mankind in general.**[42]** For Parmenides, too, what ordinary men believe is mere illusion, a world of Seeming.**[43]**

These features of early Greek philosophical writing are well known and need no elaboration. The corresponding points in relation to early Greek medical texts are in some respects more complex, reflecting in part the heterogeneity of the extant treatises, but we can give examples of both egotism and innovativeness—and their conjunction—in works of very different types. We may take first such comparatively polished or pretentious exhibition pieces,

ἐπιδείξεις

, as *On Breaths* and *On the Art* . The author of *On Breaths* repeatedly introduces himself into his text with first-person statements, specifying, for example, what he claims to be able to show,

ἐπιδείκνυμι

.**[44]** The writer of *On the Art* , who refers to himself with the first-person-singular pronoun twice in the first three lines of the work,**[45]** repeatedly parades what he represents as his personal views. For example, chapter 2 gives the author's views, introduced by "I at any rate think," about the relationship between

[42] See especially frr. 1, 2, 5, 17, 19, 29, 34, 78, 104, 108.

[43] Parmenides frr. 1.30ff., 6.4ff., 8.51ff.

[44] *Flat.* 5, *CMG* 1.1.94.6f., picked up at 15, *CMG* 1.1.101.17ff., cf. also 2, *CMG* 1.1.92.16f.; 6, *CMG* 1.1.94.8ff.; 10, *CMG* 1.1.97.12ff.; 14, *CMG* 1.1.99.20ff.

language and reality, no less,**[46]** and the following chapter offers a definition of medicine with two more first-person-singular verbs: "first I shall define what I take medicine to be."**[47]** And apart from his obsessive self-advertising, he lines himself up firmly on the side of innovation: "it seems to

me that it is the aim and function of intelligence to discover what was unknown before, wherever such a discovery confers a benefit over ignorance."**[48]**

First-person-singular statements appear with great frequency also in treatises that display a much greater knowledge of actual clinical practice. *On Airs, Waters, Places* , for instance, gives us in the opening chapter a summary of what the medical student should consider, the effects of the seasons, the changes in the weather, the effects of warm and cold winds, the properties of waters, the position of the city, and so on, and then proceeds: "I shall explain clearly,

ἐγὼ φράσω σαφέως

, the way in which each of these subjects should be investigated and what tests are to be applied."**[49]**

The severely professional *Epidemics* , especially, presents a particularly intriguing case. Thus in book 1, chapter 4, of the second "constitution," we read: "I know of no case of

καῦσος

, ardent fever, which was fatal on that occasion"**[50]** and "I have no instance to record where a cough was either harmful or beneficial on that occasion."**[51]** In chapter 8 of the third constitution: "I know of no woman who died, if these indications occurred properly, but so far as I know,

ἅς καὶ ἐγὼ οἶδα

, all who fell ill while pregnant aborted."**[52]** Again in case 4 of the set of case-histories in the same book: "I myself examined the urine which was of the colour and thickness of that of cattle."**[53]** Yet we should be

[46] *De arte* 2, *CMG* 1.1.10.10, cf. 10.2.

[47] *De arte* 3, *CMG* 1.1.10.19, cf. below Chap. 3 at nn. 27ff.

[49] *Aër.* 3, *CMG* 1.1.2.26.22f.

[50] *Epid.* 1.4 (L) 2.620.5f.

[51] *Epid.* 1.4 (L) 2.626.3f.

[52] *Epid.* 1.8 (L) 2.648.4ff.

[53] *Epid.* 1 case 4 (L) 2.692.13ff.

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careful not to conclude, from those references by themselves, that the "I" is the same individual in each case. These books generally reflect several physicians' experience: they were built up by a process of accretion and have been subject not so much to interpolation (for there is no definite original text into which interpolations have been inserted) as to a series of additions by authors who were no doubt seeking to improve their usefulness. The process may not have been too dissimilar from the way in which modern textbooks of pathology or physiology are subject to successive (if more easily identifiable) stages of rewriting and reediting in the light of what is taken to be the latest knowledge. Evidently some of those Greek doctors who inserted their contributions sometimes chose to vouch for a particular piece of information by indicating that it came from their *personal* observations. The chief point, in any event, is this: unlike the Egyptian case-histories in the Edwin Smith papyrus and elsewhere, where the authors do not intrude to vouch for their opinions or observations personally, just such claims punctuate our extant Greek clinical records.

But as in philosophy, so too in medicine first-person statements may point up claims for originality. Three treatises of rather different types that exemplify this are *On Regimen in Acute Diseases, On Fleshes* , and *On Regimen* . Thus the author of *On Regimen in Acute Diseases* , who holds forth on the shortcomings of previous writers, especially the authors and revisers of the work called *Cnidian Sentences* , begins his own positive account of diet in acute diseases with the claim: "it seems to me to be worthwhile to write down such matters as are *not yet known* to doctors even though they are of great importance and bring great benefits and injuries."**[54]***On Fleshes* draws a contrast between the opening chapter of the work, which sets out certain preliminary considerations for the study of the formation of the human body and where the author says he draws on "common opinions," and the sequel, expressing his personal views: "now I myself declare my own

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opinions."**[55]***On Regimen* , especially, describes his theory of the balance between food and exercise as a discovery,

ἐξεύρημα

, that is a "fine thing for me the discoverer and useful for those who have learnt it: and none of my predecessors attempted to understand it though I judge it to be in every respect of great importance."**[56]**

The surgical treatises, too, are often much concerned with innovation in surgical practice. On the one hand we find passages praising inventiveness**[57]** and claiming or implying that the only correct treatment is that which the author himself sets out.**[58]** On the other, these authors also frequently criticise some of their own colleagues for *their misplaced* striving after effects.**[59]** The opening chapter of *On Fractures* ,

[57] See especially *Art.* 42 (L) 4.182.13ff., which first criticises those who used succussion on a ladder (see below, n. 59) but then continues, 182.22ff.: "Yet the contrivance is an ancient one, and I at any rate praise heartily the man who first invented it—both this contrivance and any other that is in accordance with nature." Cf. 77 (L) 4.308.7ff., 10ff., which refers to the original intentions of the inventor of the method in a passage setting out an approved use of the wine-skin for reductions.

[58] Thus *Fract.* 8 (L) 3.444.1ff. specifies the correct method of reducing fractures of the humerus in contradistinction to a number of faulty methods, where the specifications are sufficiently detailed and concrete to indicate the writer's own procedures. Again at *Art.* 11 (L) 4.104.16ff., the writer states that he knows of no practitioner (we are to understand, by implication, except himself) who treats dislocated shoulders correctly. Cf. also 46 (L) 4.198.5ff., 9ff., with its general condemnation of those who claimed to be able to treat inward dislocation of the vertebrae of the spine.

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for instance, first criticises practitioners who "get a name for wisdom"**[60]** for their over-sophisticated treatments of fractured arms, and then remarks on the general problem: "Many other parts of this art are judged thus: for men praise what seems outlandish before they know whether it is good, rather than the customary which they already know to be good; the bizarre, rather than the obvious."**[61]**

This is one of several treatises in which the *new* point that the authors present as *their* contribution is, precisely, the *dangers* of the *newfangled* in medicine: texts that are trebly suggestive, first because of the indirect evidence they supply concerning the positive value set—in some quarters—on originality in medical practice, secondly because of the way they exemplify the strong authorial presence that is such a distinctive feature of early Greek medical texts, and thirdly because they illustrate how the question of *whether* or *how far* to follow tradition was openly contested.

The treatise *On Ancient Medicine* , especially, takes as its chief theme the need to return to the tried and tested methods of earlier

medical practice, what the author himself calls *ancient* medicine.**[62]** His main endeavour is to refute those who try to base medicine on the *new***[63]** method of postulates (or "hypotheses") and who thereby, in his view, drastically oversimplify the problems. Yet while allying himself with the past, he constantly uses expressions that signify his personal view. "I at any rate have not thought" (

οὐκ ἠξίουν

. . .

ἔγωγε

), he says in chapter 1, "that medicine needs a new hypothesis."**[64]** "It seems to me of the greatest importance," he says in the next chapter, "that anyone speaking about this art [medicine] should be intelligible to laymen."**[65]** Introducing his theory of the origin of medicine he begins: "I at any rate hold" (

ἔγωγε ἀξιῶ

) that it comes from dietetics.**[66]** Having explained his view of the balance of powers in the body, he proceeds:**[67]** "I think I have set forth this subject sufficiently. But certain doctors and sophists assert that it is impossible to know medicine if you do not know what a man is"—that is, his origin and elemental constitution. But that takes you into "philosophy," the kind of subject dealt with by Empedocles. "I, however,

ἐγὼ δέ

, think that what has been said or written by any sophist or doctor about nature has less to do with medicine than with painting."**[68]** In some nineteen-and-a-half

[64] *VM* 1, *CMG* 1.1.36.15–16.

[66] *VM* 3, *CMG* 1.1.37.26ff.

pages in the Heiberg (*CMG*) text, this author uses the first-person-singular pronoun no fewer than thirty times, as well as first-person-singular verbs,

φημί, οἶμαι,

and the like, without the addition of

ἐγώ

or

ἔγωγε

, on a further twenty-two occasions. His notion of the past, clearly, is *his* construction. Moreover, for all his backward looking, he has an eye also to the future, for he says he believes that the *whole* of medicine will one day be discovered, using the traditional methods he recommends.**[69]** Traditional medicine, evidently, does not yet have all the answers, even if in this author's view it shows how they are eventually to be obtained.

Several Hippocratic authors thus engage in an active debate on innovation in medical theory and practice. Their public, or at least parts of it, were evidently also much taken by the latest fads or fancies in treatment. *On Regimen in Acute Diseases* , for instance, criticises laymen for not appreciating true excellence in treatment but, rather, being preoccupied with praising or blaming strange remedies,**[70]** and *Precepts* chides patients who ask for treatment that is outlandish or obscure, though this writer puts it that they should not be punished for this prejudice, merely disregarded.**[71]** The writer of *On Joints* refers to the wonder and delight registered by patients and their friends at specially intricate techniques of bandaging (a new fad, presumably) though he goes on to remark that after a time the patients become bored with wearing their complicated bandages.**[72]**

[70] *Acut.* 2 (L) 2.234.2ff.

[72] *Art.* 35 (L) 4.158.4ff.

Today we can understand a similar twentieth-century obsession with the latest fashion in treatment in part in terms of the effects of certain

identifiable pressures, not least those from the sales forces of pharmaceutical companies. But in the ancient world there were no equivalent pressures, but just doctors trying to impress patients—or potential pupils—and patients in turn making demands on doctors.**[73]** In these circumstances the phenomenon we encounter in Greece is more surprising, particularly since to justify the use of new therapies the ancients had only insecure analogues to the argument that appeals to the full apparatus of "modern medical science."**[74]** Unlike in natural philosophy—where what was at stake was merely a matter of belief and not usually one of immediate practical consequence—the treatment a patient received from a Hippocratic or from any other type of healer might, at the limit, make the difference between death and recovery. In this context we might expect a reasonably deep-seated caution, if not conservatism, to prevail, and indeed some aspects of our Hippocratic evidence suggest just that.**[75]** Yet the degree of innovativeness tolerated, even favoured, is striking, nor is this just a matter of

[74] Cf., however, below, n. 175.

fancy bandaging, nor one where, the case being desperate, *any* remedy is worth trying. Among the surgical practices the Hippocratic doctors elaborated are some of the daunting, if not foolhardy, techniques I mentioned in Chapter 1, the forcible straightening on the Hippocratic bench,**[76]** and the succussion of the patient, often upside down, used not just in certain cases of reduction but even, amazingly, in some of difficult childbirth.**[77]** Some of those who practised succussion are accused of doing so just for the performance, "to make the crowd gape": that is the criticism made in chapter 42 of *On Joints* , although that treatise goes on itself to endorse and recommend a modified version of succussion in some cases.**[78]**

[76] See above, Chap. 1 n. 60. Who invented this and various other complex surgical procedures described in our Hippocratic texts cannot usually be determined, though Celsus provides us with a list of names (including "Hippocrates") of those who invented surgical instruments (*Med.* 8.20.4, *CML* 1.407.7ff.). There is, however, good reason to believe that the writers represented in our Hippocratic texts were themselves responsible for certain developments in surgical procedures, as, for example, when they refer to their own adaptation or modification of existing methods: see *Art.* 38 (L) 4.168.9ff., 13ff.; 78 (L) 4.312.5ff.

In the prominence of the authorial ego, the prizing of innovation both theoretical and practical, the possibility of engaging in explicit criticism of earlier authorities, even in the wholesale rejection (at times) of custom and tradition—in all these features there are marked contrasts of degree, if not also in kind, between *parts* of early Greek, and most ancient Near Eastern, speculative thought, for example, not just in styles of presentation but also in the substance of what could be presented and discussed. These are far from being the only contrasts that we might consider in relation to the study of nature, nor are they features that are confined to that general domain of inquiry. But they raise a set of problems that any evaluation of the early stages of science must confront, even if we must acknowledge that the issues stretch far beyond the range of our discussion here.

The Role of Literacy

One thesis already in the field—that aims to explain the growth of both critical and innovative attitudes, both in their ancient Near East-

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ern manifestations and in their Greek ones—has it that the key factor is the development in techniques of communication. One influential statement of such a thesis is the article by Goody and Watt entitled "The Consequences of Literacy,"**[79]** and Goody has subsequently returned to the issues on more than one occasion, modifying his thesis especially on the nature of the contrast between Greek and Near Eastern achievements and focusing increasingly on the latter.**[80]** Thus Goody argues first that many of the pre-Greek achievements depend essentially on the existence of written texts, and secondly that so far as Greece itself is concerned, the *spread* of literacy made possible by the introduction of an *alphabetic* system of writing is all-important. Literacy by itself, of course, does not discriminate between those who composed Egyptian medical texts or Babylonian astronomical cuneiform tablets on the one hand, and Hippocratic authors or pre-Socratic natural philosophers on the other.**[81]**

Many questions raised by this thesis are controversial and beyond the scope of our discussion here, notably the historical problems surrounding the development of the alphabet itself, the contributions of various Semitic groups, and the range of possible intermediaries between them and the Greeks,**[82]** as well as the thorny issues in dispute between Havelock and his opponents on the timing and extent of the spread of literacy within Greece itself.**[83]** But three points can readily be

[79] Goody and Watt 1968, cf., for example, Vansina 1971, superseding Vansina 1961/1965, and two recent collections of articles, Gentili and Paioni 1977, and Vegetti, ed., 1983.

[80] Goody 1977 and forthcoming, cf. Street 1984 and Parry 1985. The strength of the claims made for the "literacy thesis" has varied in different formulations, while it remains clear that it is invoked in order to give a causal, not merely a descriptive, account of certain changes or developments. Goody and Watt 1968 spoke of "consequences"; cf. Goody 1977, p. 75, "implications." While Goody 1977, pp. 10, 46, 51, explicitly disavows monocausal or single-factor explanations, as does Goody forthcoming, the thrust of his argument nevertheless has been effectively to discount other considerations.

[81] For one attempt to evaluate the extent and the spread of literacy in ancient Egypt, see Baines and Eyre 1983, and cf. Baines 1983.

[82] See, for example, Jeffery 1961, 1982, Snodgrass 1971, pp. 348f., Driver 1976, Coldstream 1977, pp. 295ff., Isserlin 1982.

[83] See Havelock 1963 and the essays collected in Havelock 1982, especially; cf. Kenyon 1951; E. G. Turner 1951; Davison 1962; Harvey 1966;Knox 1968; Reynolds and Wilson 1968/1974; Pfeiffer 1968, pt. 1, chap. 2; Robb 1970; Finley 1964–65/1975a, 1977; Burns 1981; Bremmer 1982; Gentili 1983. The question of the extent to which, at any period, a male citizen at Athens could do more than merely write his name should, of course, be kept separate from the issue of the role of written texts in the culture of those who numbered themselves among the most literate sections of society (cf. Finley 1983, pp. 29ff.). In Plato's day references to learning how to read and write as part of primary education are commonplace: see, for example, *Prt.* 325e, 326d, *Charmides (Chrm.)* 159c. However when Aeschines 1.6–11 suggests that primary schooling had been compulsory since Solon, this is dubious as a piece of historical evidence (cf. Havelock 1982, p. 205 n. 4), though it is certainly a pointer to what some Athenians would have liked to believe about their past. Yet even though Aristophanes *Frogs (Ranae)* 52ff. *may* allude to silent reading, such allusions are very rare in the classical period (see Knox 1968, Woodbury 1976), and it is agreed on all sides that in the fifth and fourth centuries a written text, when read, was usually read out aloud. The opening exchanges in Plato's *Parmenides (Prm.)* are among the many texts that illustrate this. There Zeno has brought his book, and Socrates, keen to learn its contents, asks him, not to lend him the text, but to read it out (*Prm.* 127c, cf. 127d–e).

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agreed. First, the existence of written texts obviously permits a different kind of critical inspection, more leisurely and more formalised than is possible with spoken discourse.**[84]** It *permits* , though it does not necessarily *dictate* , critical scrutiny, since the existence of written texts may and often does positively *inhibit* it,**[85]** a point that has been made often enough by anthropologists (for example, by Shirokogoroff in his study of the

psychomental complex of the Tungus)**[86]** and by Orientalists (for example, by Oppenheim in relation to Mesopotamian

[84] See, for example, Goody 1977, p. 149.

[85] That is to put the point negatively. Goody has the further argument, however, that there is a positive side to the limitations to a field of inquiry presented by written texts. Once the medical texts (for instance) were there for consultation, this not only preserved certain medical lore but provided a focus of attention that gave medical practitioners guiding ideas about what to look for in individual cases. Moreover, more generally, the scrupulous making and transmission of records in the ancient Near East clearly gave new depth to a sense of the past and thus transformed the notion of tradition itself, lending greater authority to appeals to the precedents it afforded: cf. Baines 1983, pp. 587ff.

[86] Shirokogoroff 1935, pp. 108, 340ff. Cf. Lévi-Strauss 1958/1968, Lotman and Pjatigorskij 1977.

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medicine)**[87]** and the validity of which for late Greco-Roman antiquity is obvious enough.**[88]** Secondly, when recorded in writing, innovations have a greater chance of being recognised as such and of being cumulative.**[89]** Thirdly, certain types of writing that are taken to be characteristic of early literacy (tables, lists, formulas, recipes) may stimulate certain types of question—for example, problems of classification—and thus affect cognitive processes themselves.**[90]**

All these are positive contributions to our understanding of these complex questions. Yet reservations about how far Goody's thesis, and others like it, go to resolve our main problems in relation to early Greek speculative thought must also be expressed. One area where some of the points Goody made can be tested, but about which he has so far had comparatively little to say in his three main discussions, is mathematics, where, as we have already noted, we have extensive Egyptian and Babylonian texts as well as Greek ones—the last beginning much later than the other two, of course.**[91]** Each of these three cultures developed its own arithmetical notation or notations, the Greek, like the Egyptian, being a decimal, the main Babylonian a sexagesimal, system. But as Goody himself has remarked,**[92]** mathematics rests on universal logographic symbols, not on restricted phonetic

[87] Oppenheim 1962, p. 104.

[88] See below, pp. 104ff.

[89] See Goody 1977, chap. 3.

[90] See Goody 1977, pp. 99ff., where, commenting on Egyptian Onomastica, Goody writes (p. 102): "We can see here the dialectical effect of writing upon classification. On the one hand it sharpens the outlines of the categories; one has to make a decision as to whether rain or dew is of the heavens or of the earth; furthermore it encourages the hierarchisation of the classificatory system. At the same time, it leads to questions about the nature of the classes through the very fact of placing them together."

[91] The evidence for Babylonian mathematics is collected in Neugebauer and Sachs 1945. For a first orientation in Egyptian mathematics, see Neugebauer 1934, chap. 4, 1952/1957, pp. 71ff.; van der Waerden 1954/1961, pp. 15ff.; among the principal primary sources are those edited by Peet 1923, Struve 1930, R. A. Parker 1972. Note the emphatic reminder in Neugebauer 1952/1957, pp. 53ff., about how much of the ancient Near Eastern, especially Mesopotamian, material remains unpublished.

[92] Goody 1977, p. 122.

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ones. In that respect all three notations are equivalent. In any case none has any *evident* superiority over the others on this score that could be compared to the superiority of an alphabetic system of writing over syllabaries, let alone over pictograms. Indeed, if there are advantages, these lie with the Babylonians, for their notation incorporated the place-value system and so revealed what the Greek use of letters for numbers conceals, namely, the equivalence of operation involved in the multiplication and division by the base and by its powers, by 10 and by 100, or, in the sexagesimal system, by 60 and 60^2 .

The fact that in all three cultures the development of complex mathematical manipulations depended on the existence of *some written* notation can certainly be taken to confirm *that* element in Goody's thesis. The role of tables here is particularly clear. Like our multiplication tables, tables of reciprocals, for instance, such as we have from Babylonia,**[93]** encapsulate knowledge that is itself decontextualised and that can be put to a variety of uses in various concrete situations. Yet Goody's thesis by itself does nothing to help explain certain major differences in the mathematics developed in these three cultures; it can hardly explain them since, as noted, the technical processes of communication involved, the notations, are in the crucial respect *broadly* equivalent.

Take one important development that is confined, so far as we know, to Greece, namely, that of the explicit notion of, and demand for, demonstration or proof.**[94]** By *proof* I do not mean the confirmation or checking of a result that regularly occurs in Egyptian arithmetic, for example, and that is often translated into English, legitimately enough, as the "proof": the scribe works his way through to the solution of a

[93] See, for example, Neugebauer and Sachs 1945, pp. 11ff. Cf. the rarer evidence for multiplication tables in Egypt, Peet 1923, pp. 103f., R. A. Parker 1972, pp. 72f.

[94] The attempts by Seidenberg (e.g., 1960–62, 1977–78, cf. van der Waerden 1980a, 1980b) to trace proofs and proof-theoretical interests back to Vedic mathematics are unconvincing, foundering on the difficulty that inadequate attention is paid to the fundamental distinction between the ability to get results, and having clear and explicit concepts of aims and methods. Cf., e.g., Knorr 1981, pp. 147f.

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problem and then checks that his result is correct.**[95]** What the Greeks eventually developed was the concept of proof in the more rigorous sense of demonstration by deductive argument from clearly identified premises: and the qualification "eventually" should be stressed, since this was a gradual and hard-won development and not a concept available to the Greeks in, say, the days of Thales or Pythagoras.**[96]** There is accordingly no call whatsoever in this respect (or indeed in any other) to speak of the Greeks as endowed with some special natural characteristic, some distinctive mental ability, as those who fantasised about the "Greek miracle" liked to do.

Moreover, in connection with the development of the concept, and practice of, mathematical proof, there is already by the late fifth century B.C. a concern with foundational problems, not the famous *Grundlagenkrisis* postulated by some historians who saw Greek mathematics as brought to an abrupt standstill when the incommensurability of the side and diagonal of the square was discovered,**[97]** but more simply an interest in the *elements* , that is, the fundamental principles

[95] See, for example, in the Rhind papyrus, Peet 1923, pp. 64f. on number 29, pp. 68f. on number 33.

[97] Against Tannery 1887, p. 98, Hasse and Scholz 1928, von Fritz 1945/1970 (who mostly favour an early date—in the early fifth, if not in the sixth century—for the discovery of incommensurability), see especially Burkert 1972, pp. 455ff.; Knorr 1975, pp. 306ff. The arguments are set out briefly in G. E. R. Lloyd 1979, pp. 112ff.

which the rest of mathematics presupposes and on the basis of which the rest of mathematics can be built up. From the very first—that is, from the work of Hippocrates of Chios, sometime around 430 B.C. —the attempt to systematise mathematics depended on decisions as to what to take *as* the elements. By the fourth century we know that Greek mathematicians were actively exploring the possibility of alternative starting-points to geometry,**[98]** though not (despite what has sometimes been claimed)**[99]** the possibility of alternative geometries: when Aristotle mentions the possibility of denying that the internal angles of a triangle add up to two right angles, it is not in connection with any proposal to construct an alternative geometry on the basis of this denial.**[100]** After Euclid, too, the question of what should be included among the axioms continued to be disputed, as the particularly well-documented and famous controversy over the status of the parallel postulate illustrates sufficiently conclusively.**[101]**

[98] In his commentary on Euclid, *In primum Euclidis Elementorum librum commentarius (In Euc.)* 66.14–68.6, Proclus refers to a sequence of mathematicians after Hippocrates and Theodorus and before Euclid himself who continued and extended work on the elements. They include Leodamas, Archytas, Theaetetus, Neoclides, Leon, Eudoxus, Amyclas of Heraclea, Menaechmus, Dinostratus, Theudius, Athenaeus of Cyzicus, Hermotimus of Colophon, and Philip of Mende. But they should not be imagined as being in fundamental agreement either about the types of starting-points necessary for the construction of mathematics or on particular issues relating to specific definitions, postulates, or common opinions. On the contrary, it is clear that there were disputes of a more than purely nominal kind on such questions. Thus those concerning the conceptions of number, plurality, and unity are discussed by Klein 1968, while the ambiguity of the term *elements* itself was remarked by Menaechmus: see Proclus *In Euc.* 72.23f.

[99] See, for example, Toth 1966–67, 1977, Bruins 1968, Hösle 1982, and cf. Kayas 1976.

[100] See, for example, *Analytica Posteriora (APo.)* 93a33ff., *Ph.* 200a16ff., 29f., *Metaphysics (Metaph.)* 1052a6f.

[101] See especially Proclus *In Euc.* 191.21ff. Elsewhere Proclus discusses the alternative definition of parallel proposed by Posidonius (176.5ff.), and Geminus' objection to the parallel postulate (183.14ff., 192.5ff.), as well as setting out Ptolemy's, and his own, attempted demonstrations (365.5ff., 371.10ff., 23ff.). See the discussion of this and other ancient evidence, as well as of aspects of the later history of the issue, in Heath 1926, 1, pp. 202ff.

None of these developments (some of course quite late) can be paralleled in extant Egyptian or Babylonian mathematics, even though, let me repeat, we can infer that they too were highly innovative in many other respects. None depends simply on technical advances in notation and the like. They do, however, all have fairly obvious affinities with the features I exemplified in early Greek natural philosophy and medicine. In mathematics, too, there is not just *manifest* disagreement between rival views and the demand for validation, but also, we may infer, a fair degree of egotism. To be sure, in this case, with fewer early primary texts at our disposal, we cannot cite extensive passages to illustrate the use of the authorial ego—not before Archimedes, at least.**[102]** Yet even in the wreck of pre-Euclidean geometry we have enough reliable evidence for the individual contributions of named theorists—for example, on special problems such as squaring the circle or the duplication of the cube,**[103]** or in the discovery of particular theorems or

[102] Archimedes often speaks in the first person, especially but not exclusively, in the prefaces to his treatises: see, for example, *Arenarius* (*Aren.*) 1.1 (HS) 2.216.15ff., 1.8 (HS) 2.220.10ff., 2.4 (HS) 2.236.8f., 3.1 (HS) 2.236.17, and cf. in the definitions and postulates in *De sphaera et cylindro* (*Sph.Cyl.*) (HS) 1.6.6, 15, 20; 8.2. And he frequently refers directly to his own discoveries and to those of other mathematicians: see, for example, *De conoidibus et sphaeroidibus* (*Con.Sph.*) proem (HS) 1.246.2ff., *De lineis spiralibus* (*Spir.*) proem (HS) 2.2.2ff., 13ff., 18ff., 4.1ff., *Quadratura parabolae* (*Quadr.*) proem (HS) 2.262.3ff., 264.4, *Methodus* proem (HS) 2.426.4ff., and cf. below, n. 104. The terminology of *sects* for rival groups of mathematicians can be illustrated in Nicomachus *Arithmetica introductio* (*Ar.*) 143.1ff. In the developing mathematical sciences, similarly, we find in the tradition of writers on harmonics, for example, some, such as Aristoxenus, who make emphatic claims for their originality: see *Harmonica* (*Harm.*) 1.1, 2–3, 4 ("No one has had any idea of these matters as yet, but in dealing with them all it has been necessary for us to make a new beginning, for we have had nothing handed down to us worthy of note"), 5–6 ("On these points no account, either with or without a demonstration, has yet been given. . . . No one has touched on this part of the subject at all as yet, except Eratocles, who attempted a partial enumeration without demonstration") 2.35–36, 37–38.

[103] The most famous of the early discussions of quadratures, after that of Hippocrates of Chios, are those of Antiphon and Bryson (mentioned by Aristotle, e.g., *Topica, Top.* 171b12ff., 172a2ff.), Hippias (to whom the quadratrix is attributed by Proclus *In Euc.* 272.7ff., 356.11, though whether this isthe sophist Hippias of Elis is disputed), and Dinostratus (see, for example, Pappus *Collectio* 4.30–32 [1.250.33–258.19 Hultsch]). In his Commentary on Archimedes' *Sph.Cyl.* Eutocius refers to several early attacks on the problem of duplicating a cube, including those of Hippocrates of Chios, Archytas, Eudoxus, and Menaechmus, (HS) 3.54.26ff., 78.13ff., 84.12ff., 88.17ff., 90.4ff. See Heath 1921, 1, pp. 220ff., 244ff.; Knorr 1986.

their proofs[104] —to be fairly confident that, like early Greek philosophers and medical writers, Greek mathematicians were often ambitious innovators and proprietorial towards their own ideas.

The Argument from Politics

Elsewhere, following Vernant and others, I have argued that *in addition to* other factors that must be held to be relevant, including the spread of literacy, the political dimension is crucial for our understanding of some of the distinctive characteristics of early Greek speculative thought.**[105]** Dealing with some Greek attacks on magic in particular, and more generally with the development of a certain openness and dialectical acuteness in parts of Greek philosophy and science (and I stressed then as I do again now that it is not the *whole* of Greek philosophy and science that can be so characterised),**[106]** I argued that

[104] Thus Archimedes scrupulously distinguishes between the discovery of the *proof* of the theorems relating the volumes of a cone and a cylinder, and those of the pyramid and prism, and the discovery of those theorems themselves. The latter, he tells us, is to be credited to Democritus, the former to Eudoxus: Archimedes *Methodus* proem (HS) 2.430.1ff., 6ff., cf. *Sph.Cyl.* proem (HS) 1.4.2ff.

[105] See J.-P. Vernant 1957/1983, 1962/1982, 1983; Vidal-Naquet 1967/1986, 1970/1986; Detienne 1967; G. E. R. Lloyd 1979, chap. 4.

these reflect the very considerable experience that many Greek citizens acquired in the evaluation of evidence and arguments in the contexts of politics and the law. True, that experience is uneven, far greater in the democracies than in the oligarchies, but it extended also to the latter on a restricted scale, even though the proportion of the population engaged in decision-taking was smaller and the occasions when they did so were fewer.**[107]** It is well known, however, that in some of the democracies that experience could be very extensive indeed, on jury service, in the Assemblies, and in any one of a wide range of offices held by lot or by election.**[108]** Certain aspects of the Greek experience of the pro-

[108] The issues have been fully discussed recently by Finley 1983, chap. 4. To mention just two of the most striking estimates he gives concerning

Athens in the fifth and fourth centuries: "the best analysis of the evidence, some of it archaeological, suggests that attendance [at the Assembly] ran to 6000 in the fifth century, to substantially more in the fourth"; "in any decade, somethingbetween a fourth and a third of the total citizenry over thirty would have been Council members, serving daily (in principle) throughout the year and for a tenth of that year on full duty as so-called *prytaneis* " (Finley 1983, pp. 73f.). Finley is also careful to point out both that "even in Athens under what modern historians tend to call the 'radical democracy', the *demos* never produced spokesmen in the Assembly from their own ranks" and that "the evidence strongly suggests that even in Athens few exercised their right of *isegoria* " (1983, pp. 27, 139f.; and cf. the reservations about the running of the democracy in R. Osborne 1985). Yet whether or not they participated as speakers in the debate, all fundamental policy decisions rested with the citizens as voters in the Assembly, just as it was they who constituted the standing bodies of jurors with whom lay the decision in the courts: see A. R. W. Harrison 1971, pp. 43ff. Cf. Hansen 1974, 1976/1983, Lanza and Vegetti 1975.

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cess Goody called the domestication of the savage mind—a process never, of course, completed, transparently not so in Greece—depend less, I would claim, on technical improvements in communication than on developments in, broadly, the political domain; they owe more to the experience that many Greeks gained there in types of argument and scrutiny than to the spread of literacy among them.**[109]**

On the more general issues of the domestication of the savage mind, I shall not repeat my earlier arguments. But there is more to be said on the specific questions we are concerned with here, where the problem presented by the material we reviewed is not so much one of innovation *tout court* , for to some degree innovation is, we said, universal. Rather, it arises from the combination of the degree of contestability of tradition and of what we may call the pressures towards *overt* innovativeness, the fashion for, even the obsession with, the novel, familiar enough to us today, but scarcely to ancient civilisations.

Now there are very evidently political dimensions to this issue too. We shall need to come back to these in due course, but three of the most obvious points may be mentioned at once. Clearly, first, innovativeness is just as prominent, or even more prominent, an aspect of Greek political life as of Greek speculative thought, and in the former is no mere theoretical matter, as new ideas could be and were put into

[109] See G. E. R. Lloyd 1979, pp. 246ff., especially 252ff., which discuss the parallelisms between the development of the notions of evidence and witnessing, testing, scrutiny, and accountability, in the contexts of law and

politics and in speculative thought.

practice in the framing and reform of constitutions and in legislative measures of every kind—a trait often disapproved of, and feared, to be sure,**[110]** but also often greatly admired.**[111]** Secondly, the possibility of dissent from deep-seated traditional views presupposes a certain measure of political freedom of speech, though that measure should certainly not be *over* estimated.**[112]** Thirdly and most importantly, the revisability of political constitutions and of the laws of various Greek city-states not only parallels, but at points interacts with, the revisability accepted in such other areas as cosmology, religion, and moral philosophy.

Thus political and moral philosophical innovation are often intrinsically related, nor can we doubt, surely, that political revisability helped to release inhibitions about revisability in other domains of thought, even while there may also be feedback from those other domains of thought to political revisability. An extended text in Aristotle's *Politics* shows that he, for one, recognised the special importance of political innovativeness in relation to other manifestations of innovativeness in other fields, for he remarked on both the similarities and the differences in this respect between politics and such arts as medicine. So far as the similarities go, he notes the argument that the advances that have been made in both domains depend on innovative-

[112] See below, at nn. 187–88.

ness, putting it, very much with the voice of Greek rationalism, that all men seek the good rather than the traditional.**[113]** At the same time he points to a contrast, in that politics deals with what is established not just by rules but by custom, so that frequent changes in the laws have the effect of weakening the capacity of the law. Thus in this regard "much caution is necessary."**[114]** But the very contrast suggests that, like many other Greek theorists, Aristotle saw politics as a master art that controls the very framework within which the other arts are exercised. The reason why particular caution is needed as regards innovation in politics is, precisely, that it has such far-reaching repercussions.

To throw light on the problems presented by both the positive and the negative aspects of the pressures towards overt innovativeness in Greece we

may investigate some of the *contexts* in which it is mani-

fested. In Chapter 1 I broached some aspects of the growth and transformation of rivalry in claims to wisdom,

σοφία

, and some points should now be elaborated further.

Σοφία

and the Sophistic Debate

Both in the archaic and the classical periods the term

σοφία

had, as is well known, an enormous range.**[115]** It is often and foremost skill in poetry that is in question.**[116]** But in the classical period you can be called

σοφός

in any one of the arts, painting or sculpting or flute-playing, in athletic skills, wrestling, or throwing the javelin or horsemanship, and in any of the crafts, not just in piloting a ship or healing the sick or farming but, at the limit, in cobbling or carpentry or cooking: all those examples can be illustrated from the Platonic corpus.**[117]** From the seventh century onwards, many different kinds of leader gained a repu-

[116] See below, n. 128.

tation for *sophia* in general. They included seers, holy men, wonder-workers. Men such as Epimenides, Aristeas, Hermotimus,**[118]** were consulted in crises or disasters, plagues or pollutions,**[119]** which shows how wise men may be not just spokesmen of traditional lore but called in where that

knowledge faces an impasse—though, to be sure, in offering a way forward any wise man may represent himself as the true exponent of tradition as much as the mediator of knowledge that goes beyond the common store. But already in the sixth century the variety of

σοφοί

is considerable. Among those who appear in the lists of the Seven Wise Men (starting with Plato's)**[120]** those who had a reputation as statesmen figure prominently: they include Solon, Pittacus, and Periander, and it is possible that Thales won his place in their number as much for the political advice he gave his fellow Ionians**[121]** as for his ideas about water as the origin of things—which, as is well known, are formidably difficult to interpret with any confidence, being, indeed, a matter of conjecture already for Aristotle.**[122]**

[119] Consultations not just of oracles, but also of those deemed to be experts in religious matters, for example, when pollution has occurred or is suspected, continue to be referred to in fifth- and fourth-century texts, as is clearly illustrated in Plato's *Euthphr.* 4b–c and 9a. See R. C. T. Parker 1983, p. 141.

[120] Plato *Prt.* 343a, cf. *R.* 335e f. D.L. 1.41f. records contrasting traditions concerning the membership of the Seven.

[121] See, for example, Herodotus 1.170 (advice to set up a common centre of government), D.L. 1.25, and compare the stories relating to his practical skills, Herodotus 1.75 (diverting the river Halys), Aristotle *Pol.* 1259a6ff. (forecasting a bumper crop of olives and cornering the presses). When at *Hp.Ma.* 281c, Socrates says that most of the early wise men down to Anaxagoras kept clear of politics, this is more than a little ironical: he has just referred to Pittacus and Bias, as well as Thales.

The existence of more or less formalised competitions in "wisdom" of one kind or another from as early as the eighth century B.C. provides us with a clue concerning the *eventual opening* for the inquiry concerning nature.**[123]** I noted that Hesiod already tells us that he won a poetry competition and that there is a similar allusion to such a competition in the Homeric *Hymn to the Delian Apollo* .**[124]** Trials of skill at solving riddles,

γρῖφοι

, are referred to not only in the legend of Oedipus, but in our evidence for such admittedly shadowy figures as Mopsus and Calchas,**[125]** and riddles did not just

remain a feature of oracular discourse,**[126]** for the ability to resolve them continued to be, in popular legend, a mark of the wise man.**[127]** Xenophanes provides pre-

[124] See above, n. 29. On the apocryphal story of the competition between Homer and Hesiod, see West 1967, N. J. Richardson 1981.

[125] Hesiod fr. 278 Merkelbach and West: cf. the material collected in Ohlert 1912 and in Schultz 1914, and cf. Veyne 1983, pp. 41ff.

[126] As is recognised directly in, for example, Herodotus (e.g., 1.53ff. and 71), and indirectly in the parodies of Aristophanes, e.g., *Peace* (*Pax*) 1070ff., *Birds* (*Aves, Av.*) 960ff.

[127] Thus it is a recurrent theme in Diogenes Laertius that the "wise men" or "philosophers" whose lives he records show a particular ability both to formulate and to resolve riddles or puzzling questions of one type or another.This can be illustrated not just in his accounts of the proverbial wise men in book 1 (Thales, 35f.; Chilon, 68f.; Pittacus, 77f.; Bias, 86f.; Cleobulus, 89) but in later books as well (for example, Aristippus, 2.68ff.; Aristotle, 5.17ff.; Theophrastus, 5.39f.). That wisdom may be enigmatic is a theme in Plato *Chrm.* 161c, 162a–b, 164e ff., cf., e.g., *Alc.* 2.147c–d. It may not be too farfetched to recall that some of the classic problems in Greek mathematics are posed in the form of riddles, for example, the Delian problem of the duplication of a cube (see Theon 2.3ff., Eutocius [HS] 3.88.5ff.) and Archimedes' Cattle Problem in the work of that name, (HS) 2.528.5ff.

cious early evidence of rivalry (though not in a formal

ἀγών

) between different types of claimant to excellence when in fr. 2 he complains that the useless achievements of athletes are prized more than *his sophia* —where he speaks, no doubt, both as statesman and poet in general and as someone involved in the investigation into nature in particular.**[128]** We may recall, too, Heraclitus criticising others for "much learning,"

πολυμαθίη

:**[129]** in one of the particular contexts in which he attacks Homer, Heraclitus specifically calls him "wiser than all the Greeks,"**[130]** and he expresses his

contempt also for the "bards of the peoples" (

δημῶν ἀοιδοί

),**[131]** while reserving the title of "the wise" for his own teaching,**[132]** including his own teaching about the true Zeus.**[133]** Here, then, a space could be won for philosophy, including the kind we call science, in an area already associated with poetry and religion.**[134]**

[129] Heraclitus fr. 40; see above, n. 41.

[131] Heraclitus fr. 104: "For what mind or sense do they have? Not knowing that 'the many are bad, the good few,' they believe the bards of the peoples and use the mob as teacher."

[132] Heraclitus frr. 41, 50, 108.

[133] Heraclitus fr. 32: "The one wise thing is not willing, and is willing, to be called by the name of Zeus."

The "wise man" thus afforded some of the early cosmologists a category within which to work, one that was flexible enough to *permit* innovation.**[135]** Thus far, extensive parallels for at least some of what we know about Greece can be found in other societies, for instance in the competitions in riddle-solving or in other aspects of wisdom reported from India, Sumeria, Babylonia, and many other parts of the Near East.**[136]** But one important eventual difference in degree, if not in kind—as, again, others before me have stressed**[137]** —is that *some* of the Greek competitions were a matter of *public* debate, adjudicated by lay audiences with (as we noted) considerable experience in evaluating arguments in such other contexts as the Assemblies and the law courts. In India, the contests reported in the Upanisads**[*]** , at least, are essentially esoteric.**[138]** It was, in general, up to the wise men themselves to claim

[136] On the wisdom literature of the ancient Near East, see especially van Dijk 1953, Lambert 1960, Bottéro 1974, 1977 (for Mesopotamian versions of a tradition of Seven Sages, though not as historical personages, see Reiner 1961; Bottéro 1981b, pp. 110f.). On riddles in general see Huizinga 1944/1970, pp. 127ff.; Dundes 1975, pp. 95ff. On riddling and riddling games in ancient Indian sacred literature in particular, including the Rgveda**[*]** , see especially Winternitz 1927, pp. 117ff., 183ff., 352ff.; Gonda 1975, pp. 132ff., and cf. pp. 379ff. on the brahmodyas.

[137] See especially J.-P. Vernant 1957/1983, 1962/1982, 1983; Vidal-Naquet 1967/1986; Detienne 1967, chap. 5.

[138] This appears to apply both to the Chandogya**[*]** Upanisad**[*]** and to the Brhadaranyaka**[*]** Upanisad, as following Ruben 1929 and 1954, chap. 8, I argued in G. E. R. Lloyd 1979, pp. 60f. That, in some sense, the debate between Yajñavalkya**[*]** and other sages in the Brhadaranyaka Upanisad is one between new and old wisdom (as Ruben 1979, p. 150, suggests) does not substantially alter the esoteric, specialised nature of the discussion.

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victory or to acknowledge defeat.**[139]** As for the ancient Sumerian wisdom debates, recently adduced by Frischer as parallels to the Greek material,**[140]** in them the judgement of the contest is represented as in the hands of a god—Samas**[*]** or Enlil.**[141]** However we *interpret* what that verdict means, it is firmly assigned to non-human authority.

In the Greek context, the speakers often addressed, and had to be intelligible to, a far wider public. The author of *On Ancient Medicine* , as we noted, insisted that it is "of the greatest importance that anyone speaking about this art should be intelligible to laymen."**[142]** But that in turn *involved* the layman, in that case in making up his mind about medical theory, in others about physics or cosmology, in yet others

[139] Thus in the debates in the Brhadaranyaka**[*]** Upanisad**[*]** , the sages yield to Yajñavalkya**[*]** and acknowledge defeat by falling silent (e.g., Hume 1931, pp. 109–19). While various types of speaker are there involved, the audience as such plays no direct part: at one point, indeed, Yajñavalkya takes another sage aside and says, "We two only will know of this. This is not for us two (to speak of) in public" (Hume 1931, p. 110). Elsewhere, however, Gonda 1975, p. 380, notes an implicit or passive role for the audience when, for example, in the Brahmanas**[*]** the author "after stating two different opinions, lets his audience take their choice"; cf. Renou and Silburn 1949a, pp. 22ff. Cf. Keith 1928, p. 408, on discussions at the Sabhas**[*]** held by rich kings or patrons: "any new doctrine which desired to establish itself was only able to do so if its supporter could come forward on such an occasion and by his advocacy secure the verdict of those assembled and the favour of the king or patron of the assembly."

[140] Frischer 1982, pp. 14f., drawing on van Dijk 1953. Yet van Dijk, p. 39, specifies that the judgement is given by a god (cf. pp. 49f.) and while the story of Enkimdu and Dumuzi is an exception to this general rule, even there they both address Inanna Queen of Heaven.

[141] See, for example, Lambert 1960, pp. 150f. Lambert notes that "there

is no certainty that a judgement did take place in all the Babylonian texts," though there are traces of such scenes both in *Nisaba and Wheat* and in *The Fable of the Fox* (where Samas is involved, p. 201).

[142] *VM* 2, *CMG* 1.1.37.9f., see above, n. 65. *Vict.* 3.68 (L) 6.594.3ff., cf. 598.4ff., explicitly addresses itself to "the many," as also does *Salubr.* 1 (*Nat. Hom.* 16), *CMG* 1.1.3.204.22ff. In the question-and-answer sessions envisaged in *Morb.* 1.1 (L) 6.140.1ff. (see below at nn. 177 and 180) laymen as well as doctors are involved. Again *De arte* 4, *CMG* 1.1.11.5f., perhaps echoing Diogenes of Apollonia fr. 1, demands that the starting-point should be agreed by all—where, presumably, "all" is not restricted to medical theorists.

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about aspects of morality or even religion.**[143]** There are still rules about winning and criteria for success, but in principle, at least, those rules are entirely open. They are not made to depend on the authority of individuals, human or divine, let alone on the authority of some corporate notion of the past or of what is hallowed by tradition, even though what they do depend on chiefly, the appeal to *logos* , still cannot be totally dissociated from those who purported to be its representatives. In medicine that meant most of the Hippocratics, though other Greek healers, those in the temples of Asclepius or the itinerant purifiers,**[144]** no doubt refused to enter that kind of debate, to play the

[143] The lay public were, in different contexts, called on to exercise their judgement on a wide variety of topics, not just as the jury in the dicasteries and as voters in the Assemblies (which Euthyphro describes himself as addressing on religious matters, Plato *Euthphr.* 3b–c). Judges chosen by lot, or the audience as a whole, decided many poetry, drama, and music competitions. At *Lg.* 659b–c Plato remarks on this as a current custom in music competitions in Sicily and Italy, and there and elsewhere does not conceal his contempt for the practice (*Lg.* 658a ff., 700c ff., *R.* 492b, cf. Aristotle *Poetica* [*Po.*] 1451b35ff., *Pol.* 1281b7ff.). Moreover, it was the bystanders who adjudicated the type of *epideixis* competition referred to in the Hippocratic *Nat.Hom.* (see below at nn. 156 ff.)—in that case, a debate on the elemental constitution of the human body. Again, Plato frequently makes play with the hubbub and applause that might greet a sophist's speech or eristic questioning, e.g., *Prt.* 339d–e, *Euthd.* 276b–d, and the caricatures that he offers of the styles of Prodicus and Hippias, for instance (*Prt.* 337a ff., c ff.), might be taken to suggest that once a sophist became known for a particular set of mannerisms he might well become the prisoner of his own reputation and find it hard not to give the audience what it had come to expect (cf. also the association between writers of manuals on rhetoric and the particular tropes they invented at *Phdr.* 266d ff.). Plato himself, of course, repeatedly emphasised that what mattered was not the verdict of the crowd, but the truth (e.g., *Grg.* 471e, 472b–c, 474a). The

continued role of the crowd as judges in music and other competitions emerges from, for example, Lucian *Harmonides* (66) 2f., and Galen several times refers to competitive public anatomical dissections in front of an audience quick to ridicule failure, e.g., *AA* 7.10 (K) 2.619.16ff., 7.16 (K) 2.642.3ff., 645.7ff., cf. *CMG* 5.8.1.96.9ff., 98.9ff. ([K] 14.627.5ff., 629.1ff.), and see Vegetti 1981, pp. 54ff.

[144] The evidence from the temple inscriptions at Epidaurus certainly suggests that those who set them up were keen to claim *efficacy* for the god's healing (cf. below, Chap. 3 at n. 112), and at one point the divergence between thegod's treatment and what ordinary mortals would prescribe is underlined. This is in case 48, Herzog 1931, p. 28, where the first instruction the god gives to the patient is that he should not follow the treatment (cauterisation) recommended by ordinary doctors. Similarly, later, Aelius Aristides refers often enough to the god overruling the diagnoses or therapies of ordinary physicians, e.g., *Or.* 47.61–64, 67–68, 49.7–9, where the god is always right. But while, implicitly or explicitly, differences between styles of treatment, and especially in their comparative success, are noticed, there is nothing to suggest that the healers at the temples of Asclepius or the itinerant purifiers attacked in *On the Sacred Disease* chose to *debate* with other doctors such questions as the causation of diseases, the constitution of the body, and the right type of treatment and its justification, along the lines of the discussions presupposed in such Hippocratic works as *Flat., De arte, Nat.Hom., Morb.* 1 and *VM* , let alone in the debates on the foundations of medical method in the Hellenistic sects.

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game by the rules those Hippocratic writers themselves laid down. The new wisdom did not, of course, drive out the old, in medicine especially, though it evidently proved its attractiveness *at least to a certain kind of audience* of those keen, in principle, to judge what was said by the case made out for it, rather than just by the standing of the speakers.**[145]** The double bind on the new-style wise men was that they sought to be not just admired (like athletes) but understood, even while they insisted that what they offered to be understood was no merely common understanding.

In the open debates that we know took place each participant, striving to win, would naturally try to justify his own position and undermine those of his opponents, and one way he might attempt to claim superiority for his own ideas was by stressing their novelty. Moreover, the occasions for display that occurred (both in connection with contests of wisdom and independently of them) did not just permit, but must sometimes positively have *favoured* open, indeed ostentatious, claims to originality. We know, for example, that the pan-Hellenic games provided one context not just for music, drama, and poetry

[145] This is not to deny that the standing of the speaker must often have played a role in the evaluation of his performance in an *epideixis* or a debate. Indeed, Aristotle was explicitly and repeatedly to emphasise the importance of the apparent character of the speaker as a factor in his carrying conviction with his audience: *Rh.* 1356a1ff., 1366a10ff., 23ff., 1377b24ff., 1378a6ff.

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competitions, but for other intellectual exhibitions—part education, part entertainment—of various types,**[146]** including lectures not only on morally uplifting or cultural subjects but even on such topics as element theory or the fundamental constituents of the human body.**[147]** We hear, for instance, of the frequent attendance of Hippias at the Olympic games. He took along, according to the evidence in Plato's *Hippias Major* and *Minor* ,**[148]** his own poems and prose works and was ready to speak on any subject on which he had prepared an exhibition piece or *epideixis* , and to answer questions afterwards.**[149]** Gorgias, too, we know, gave speeches at the Olympian and the Pythian games, and according to Plato was prepared to answer questions on any subject anyone cared to propose.**[150]**

Most of our specific evidence relates, to be sure, to well-known sophists**[151]** such as the two just mentioned, and accepting the sophists as making any serious contribution to developments relating to science still presents difficulties, since discussion still continues to be concentrated rather narrowly on the work of a small group of the most famous individuals, beginning with Protagoras, and to be preoccupied with the criticisms that Plato brought against them. These criticisms pose a major obstacle to our understanding, since he figures so promi-

[146] This is clear, for example, from the evidence concerning Hippias; see below at n. 149, and cf. n. 166. The festivals continued to be the, or a, context in which the dream-interpreter might hope to attract a public, as we may infer from Artemidorus's references to frequenting them to collect dreams, 1 pr. 2.18, 5 pr. 301.10f.

[147] See below at nn. 156ff. on *Nat.Hom.*

[148] The continuing debates on the authenticity of these works do not materially affect their usefulness as evidence of the type of interests Hippias was believed to have.

[149] See especially *Hp.Mi.* 363c–364a, 368b–369a; cf. *Hp.Ma.* 281a ff., 282d–e, Philostratus *Vitae Sophistarum (VS)* 1.11.7.

[150] See, for example, Plato *Men* . 70c, *Grg.* 449b–c, Aristotle *Rh.* 1414b29ff., and cf. Cicero *De finibus* 2.1.1, Philostratus *VS* 1.9.4ff.

[151] Among the more important recent discussions of the sophists have been those of Guthrie 1969; Stanton 1973; Welskopf 1974; Vlastos 1975b, pp. 155ff.; Martin 1976; Classen, ed., 1976; Moreau 1979; Kerferd 1981; and Kerferd, ed., 1981.

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nently among our early sources of information.**[152]** Yet we must recognise that there was far more to what is called the sophistic movement than the work of the named individuals Plato attacks or even of the generality he abuses. The category of sophist, in Plato himself, as well as elsewhere,**[153]** is far from hard-edged, and there were important over-

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laps not only between sophists and natural philosophers but also and more especially between sophists and medical writers or lecturers.**[154]** There is a permeability in those categories, as well as a permeability in the audiences the individuals concerned took as their targets.

Certainly the extant medical texts yield excellent evidence that

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some medical writers fought hard to differentiate themselves both from those *they* called "sophists" and from what they call "philosophy." The writer of *On Ancient Medicine* , as we saw, emphasises the point when he rejects speculative theorising about the nature of man, where he specifies that it is not just what he calls sophists but also doctors,

ἰητροί

, who are at fault.**[155]** We can examine one example of such theorising in the treatise *On the Nature of Man* . The author of the first eight chapters of that work may well have been a medical practitioner himself,**[156]** but he has more than a touch of the sophist about him too,**[157]** even though he is also concerned to

distinguish the way he treats the subject of the nature of man from the way other lecturers do when they go beyond what is, in his view, relevant to medicine. Those lecturers can be seen to be ignoramuses, the writer says,**[158]** among

[155] See *VM* 20, *CMG* 1.1.51.6ff. and 12ff., quoted above, nn. 67–68.

[156] *Nat.Hom.* is fairly clearly a composite work, and the fact that the blood-vascular theory of 11, *CMG* 1.1.3.192.15ff., corresponds to that ascribed to Polybus by Aristotle *HA* 512b12ff., and, further, that the report of Polybus' theories in Anon. Lond. 19.1ff. tallies broadly with the views set out in *Nat.Hom.* 1–8, does not necessarily mean that we should attribute the whole of the treatise as we have it to him, though that cannot be ruled out.

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other reasons because the same man never wins the argument three times in succession, but whoever happens to have the glibbest tongue in front of the crowd: important, if well known, evidence for the existence of those competitive lectures on physiology judged by a lay audience.**[159]** It is clear that some of the medical texts that seek to distance themselves from those that offered general disquisitions on topics marginally relevant to medicine have more in common with the works in question than might seem likely from the way they set out to stress the distance.**[160]** Moreover, as we noted earlier, there are other texts that do not dissociate themselves from the sophistic *epideixis* , but *follow* that model and exemplify it. We have mentioned *On the Art* and *On Breaths* as the two most striking cases.**[161]** The reaction of an earlier generation of commentators was to suggest that maybe either Protagoras or Hippias himself wrote *On the Art* .**[162]** That is unlikely, in all conscience, but it was certainly not just foolish of Theodor Gomperz and others to entertain such a possibility.

On Breaths and *On the Art* may be taken to establish that the sophistic *epideixis* marks one extreme end of the spectrum that our extant Greek medical treatises represent. Those treatises do form a *spectrum* : there are important distinctions to be drawn between more, and less, popular works, between general lectures and practical manuals or

[159] *Nat.Hom.* 1 envisages those who attempt to prove that man consists of a single physical element, air, water, fire, or earth, but 2, *CMG* 1.1.3.166.12ff., attacks doctors who similarly argue that man is constituted by blood, bile, or phlegm alone.

[160] Thus while *Nat.Hom.* 1–8 attacks monistic physiological and pathological theories, the pluralist doctrines the writer himself recommends are based on similar ideas concerning the elements in the body and the causes of diseases: see below, Chap. 3 at nn. 42ff.

[161] See above at nn. 44f.

[162] See T. Gomperz 1910, pp. 22ff.; W.H.S. Jones 1923–31, vol. 2, p. 187.

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collections of notes (for instance), between authors with more, and less, clinical experience, or with none at all. But the extension of that spectrum *as far as* the sophistic *epideixis* can throw light on at least some aspects of our specific problem concerning what I called the pressures towards overt innovativeness. If we turn back to the sophistic *epideixis* , three features stand out. First and most obviously, in the context of an exhibition performance, whether at one of the great Games or on some other public occasion, caution and reserve are not likely to be the most highly prized qualities. On the contrary, every effort will be made to attract and hold an audience, to make the "sales pitch" as effective as possible. This was, after all, one of the chief ways in which teachers attracted fee-paying pupils.**[163]** We expect, and we duly find, in examples of the genre, both from Gorgias and in the Hippocratic Corpus, a striving after originality as well as after effects of every kind.**[164]**

Secondly, from the side of the hearers, we may imagine that most were aware of, and so must surely to some extent have discounted, the elements of exaggeration in this kind of performance. Although the analogy should certainly not be pressed too far—and maybe Huizinga did press it too far**[165]** —the occasion of the sophistic *epideixis* has some of the razzmatazz of the fairgound sideshow.**[166]** Most of the audience at Delphi and Olympia were away from home, and all must have been

[163] Thus we hear from Plato that Prodicus had both a fifty-drachma and a one-drachma *epideixis* on the correctness of names (*Cratylus, Cra.* , 384b–c) and Aristotle at *Rh.* 1415b15ff. writes of his recommending slipping in sections from the fifty-drachma exposition when the audience showed signs of nodding off.

[164] Thus when Diodorus 12.53.2–5 records the sensation that Gorgias created when he visited Athens on an embassy from Leontini (in 427 B.C.), this is explicitly attributed, in part, to the novelty of his rhetorical style. Again in Xenophon *Mem.* 4.4.6–7, Hippias expressly claims to attempt to say something "new" on each occasion, while later Isocrates also claimed originality for his own work: 9.8–11, cf. 4.7–10, 12.10ff. Similarly, Lysias' speech is explicitly praised by Phaedrus in Plato's *Phaedrus* (227c5ff.) for its subtlety and inventiveness.

[165] Huizinga 1944/1970. Compare Bakhtin's discussion of the ancient

origins of what he calls the carnivalisation of literature: Bakhtin 1973, pp. 87, 93, 100ff., and cf., e.g., Carrière 1979.

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conscious of the contrast between festival and everyday experience. Certainly the element of playfulness is commented on directly by Gorgias at the end of his *Helen* .**[167]** In Thucydides too, when in the Mytilenean debate Cleon is made to chide the Athenian Assembly for their lack of seriousness, he puts it that they are behaving like an audience at a performance of sophists,**[168]** and Thucydides himself underlines the seriousness of his own historical enterprise by insisting on the contrast between it and the competition pieces of earlier writers of chronicles,

λογογράφοι

.**[169]**

The point extends to the inquiry concerning nature and to medicine. Exploiting what became a standard device to put down opponents, the author of *On Ancient Medicine* contrasts his own serious interests in the art with their speculations, which, in his memorable phrase, belong rather to painting than to medicine.**[170]** Plato too undercuts all attempts at accounts of the changing world of becoming—his own included—by categorizing them as a mere pastime,

παιδιά

, even though a moderate and intelligent one.**[171]** We cannot represent the

[167] Gorgias *Helen* (fr. 11) 21; cf. Aristotle's report at *Rh.* 1419b3f. (fr. 12) that Gorgias recommended that one should destroy one's opponents' earnestness with laughter, and their laughter with earnestness.

[168] Thucydides 3.37ff., especially 38.4 and 7.

[169] Thucydides 1.21–22.

[170] *VM* 20, *CMG* 1.1.51.12ff., cf. above, n. 68.

[171] Plato *Ti.* 59d. Plato further undercuts the seriousness of writing as a whole, notably in passages in *Phdr.* , e.g., 274b ff., 276a–d, 277d–e, and

Ep. 7.341b ff., much discussed by, for example, Gaiser 1963, pp. 3ff; Gadamer 1964/1980, 1968/1980; and Havelock 1963, 1982. On the other hand, the lack of seriousness is a charge repeatedly brought against eristic or antilogic, e.g., *Euthd.* 278b ff., 283b–c, 288b–c, *Grg.* 500b–c, *R.* 539b ff., *Sophist (Sph.)* 251b–c, *Philebus (Phlb.)* 15d–e. Aspects of the interactions of the serious and the playful in Plato have been discussed from different points of view by, for example, de Vries 1949; Friedländer 1958, vol. 1, chap. 5; Derrida 1972/1981, pp. 65ff.; Brisson 1982; Ferrari forthcoming.

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whole of early Greek science just as fun—though that would suit Feyerabend as well as Huizinga.**[172]** At the same time we should not ignore what the signs of speculative playfulness in parts of it can tell us about the aims of authors and the expectations of audiences.

Thirdly and more generally, it is worth emphasising that some of our Greek material relates to contexts, such as inter-state games, where at least some of the constraints that existed within any given city-state were suspended—though not all were, and some additional ones were operative. Again, of the main groups of "intellectuals" concerned, nearly all of the most notable "sophists," many of the doctors, and indeed quite a number of the natural philosophers too had spheres of influence that were not confined to a single state. They could and did move freely from one city to another, both simply to earn their living and, if need be, to avoid political trouble. That is, after all, what Aristotle was to do when he withdrew from Athens in 323 B.C. , and it was probably what most people expected of Socrates.**[173]** Here too the link with politics is clear, and while this aspect of political pluralism no doubt facilitated, and may even have been a necessary condition of, intellectual innovativeness, we should not underestimate the possible influence in the reverse direction, the effect that such intellectual innovativeness could and did have on the development of political pluralism.**[174]**

Far more than their counterparts in most other ancient civilisations, Greek doctors, philosophers, sophists, even mathematicians, were alike faced with an openly competitive situation of great intensity. While the modalities of their rivalries varied, in each the premium—to a greater or lesser degree—was on skills of self-justification and self-

[172] See Feyerabend 1975 and 1978.

[173] When news of Alexander's death reached Athens in 323 B.C. , this sparked off a wave of anti-Macedonian feeling and Aristotle was charged with impiety, on the ground that he had composed a hymn to Hermias, according to D.L. 5.5f., whereupon he withdrew to Chalcis, where he died in the following year. The expectation that Socrates would accept the help of

his friends to escape from prison before he was executed provides the dramatic setting of Plato's *Crito* , but was clearly no merely dramatic device.

[174] See, for example, Finley 1973a, chap. 3, 1983, pp. 123ff.

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advertisement, and this had far-reaching consequences for the way they practised their investigations as well as on how they presented their results. To be sure, to stress the novelty of your own ideas was not the only possible tactic to adopt in such a situation. Some medical writers, as we saw, took the opposite stance, criticising newfangled theories and treatments and siding with what they represent as traditional methods: yet we also found that when the author of *On Ancient Medicine* takes that line, his arguments—and it is to be noted that he does *argue* —are punctuated by expressions that underline his own authorial presence. The temptation to claim to introduce new ideas and practices was there and often not resisted—it was, indeed, yielded to with some abandon. As I remarked earlier, new medical treatments could not be justified with appeals to the authority of "modern medical science"—to the outcome of laboratory tests and the like—even though some ancient doctors were certainly not above attempts to mystify their clients with esoteric talk of the supposed humoral or elemental analysis of drugs and other therapies.**[175]** But since there were no legally recognised medical degrees or qualifications for them to cite as basic credentials, they had to start further back, as it were, and rely more on the force of direct argumentative persuasion to get remedies, new or old, accepted.

To win and hold an audience demanded a strong personality and the gift of the gab, whether in the surgery or in the public lecture or debate: while those contexts no doubt look very different to *us* , in Greece they were readily connected.**[176]** You could not, or at any rate you did not, if you were an exponent of Hippocratic rationalism, simply

[175] The rationalist doctors prided themselves precisely on being able to explain the effects of drugs and were not content with merely empirical remedies in the modern medical sense of that term; but such theories as they produced usually took the form of highly speculative, if not quite arbitrary, appeals to opposites, elements or humours.

[176] Thus *Praec.* 12, *CMG* 1.1.34.5ff., warns against turning a consultation into a public lecture or occasion for display. ("And if for the sake of the crowd, you wish to hold a lecture, your ambition is no laudable one; but at least avoid citations from poetry, for that betrays an incapacity for industry.") Cf. *Decent* . 2, *CMG* 1.1.25.15ff.

shelter behind, or assimilate yourself to, the "tradition"—at least not without justifying your so doing. We know from the treatise *On Diseases* book 1,**[177]** as well as from a famous text in Plato's *Laws* ,**[178]** —that some doctors might expect to have to justify their diagnoses and treatments not just to other doctors (behind the scenes, as it were), but to and in front of their patients and their patients' friends and relatives, sometimes with other doctors present seen as rivals eager to take over the case if the opportunity arose.**[179]** Thus *On Diseases* 1 provides tips on how to deal with the veritable cross-examination you might have to withstand.**[180]** There was no deference to the professional in the white coat. Yet externals were not irrelevant. Another Hippocratic work, *Precepts* , warns the doctor against the use of luxurious headgear and exotic perfumes to impress: that clearly indicates where the temptations lay, even though this particular author says firmly that they should be resisted.**[181]**

The natural philosophers did not similarly have to amaze their audiences to get them to take their medicine. They were not competing for patients, though they were for pupils—and fame. Yet Empedocles at

[177] See *Morb.* 1.1 (L) 6.140.1ff.; cf. also *Decent* . 3, *CMG* 1.1.25.20ff., 25ff.; 12, *CMG* 1.1.28.25f.; again at *Art* . 1 (L) 4.78.1ff., 9ff., the author refers to a discussion of the diagnosis and treatment of dislocated shoulder in which both doctors and laymen are involved.

[178] Plato *Lg.* 720a ff., cf. 857c ff.; see Kudlien 1968c, R. Joly 1969–70, Hosek**[*]** 1973.

[179] In the Hippocratic Corpus, *Praec.* sees fit to recommend that the doctor should not be reluctant to call in other doctors, if need be, for consultation and warns that there should be no jealousy between doctors (8, *CMG* 1.1.33.5ff., 12ff.; cf. *Praec.* 7, *CMG* 1.1.32.22ff.). Galen later cites several cases where he takes over from other doctors when their treatment (according to Galen) brought no results, and he even relates how he countermanded other doctors' orders without their knowing: see, for example, (K) 10.536.11ff., 538.12ff., cf. (K) 11.299.10ff., *CMG* 5.8.1.82.25ff. ([K] 14.614.9ff.); cf. Nutton 1979, p. 169.

[180] *Morb.* 1.1 (L) 6.140.1ff., especially 142.7–12. Once again there is a parallel in the literature concerned with dream interpretation, for Artemidorus too offers advice about how to deal with the questions put to the interpreter, 4 pr. 237.25f., 4.84,299.15ff.

least, who was the only prominent early cosmologist, so far as we know, to have some of his work delivered by a rhapsode at Olympia,**[182]** was certainly not easy to outdo in showmanship. "He liked," as Guthrie put it,**[183]** drawing on Diogenes Laertius, "to walk about with a grave expression, wearing a purple robe with a golden girdle, a Delphic wreath, shoes of bronze, and a luxuriant growth of hair, and attended by a train of boys."**[184]** But although the styles and contents of their speculations differ widely, what Anaxagoras, Democritus, and the rest have in common with Empedocles is that explicitly or implicitly they too claim to have found the solutions to physical and cosmological problems that had defeated everyone else. However much they differ in their other interests, they were rivals there, and were in business to argue that their own ideas were different from, and superior to, everyone else's.

Both in medicine and in natural philosophy the written text had an important and, as time went on, an increasing role as the object of critical reflection, though (as we noted) the texts, when read, were still usually read out.**[185]** Yet overt innovativeness in speculative thought and the corresponding self-distancing from tradition stem not only from the spread of literacy (by itself no guarantee that such attitudes will be adopted), but also from a complex, pluralistic social and cultural situation. What may be particularly important there is the development of new modes of rivalry and competition, calling for new styles of self-justification. In philosophy too, as in medicine, the individual

[182] D.L. 8.63. At D.L. 2.10 a different type of success is ascribed to Anaxagoras, when he went to Olympia: he wrapped himself up in a leather cloak as if it were going to rain, which it duly did. Diogenes recounts other stories of the visits of other wise men at Olympia (e.g., Plato, 3.25; Diogenes the Cynic, 6.43) and while these may well be apocryphal, the idea of such visits is not implausible. Cf. also Lucian *Herodotus* (62) 1ff., who reports that Herodotus went there to have his history recited.

[183] Guthrie 1965, p. 132.

[184] D.L. 8.73, cf. 66. Aelian *Varia Historia* 12.32 refers to Hippias and Gorgias also wearing the purple robes that were associated particularly with rhapsodes.

[185] See above, n. 83.

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often thought of himself as participating in—and sometimes literally participated in—a debate in which the personal contribution of each participant was clearly marked as *his* , even when he did not go out of his way (as so many did) to stress his originality explicitly.**[186]** When we speak of Greek writers needing to win and hold an audience, *audience* is often the apposite term, and it may be to that interaction with audiences, and to the development of contexts for that interaction, that we have to look for the chief clues to the understanding of the particular positive and negative modalities of innovativeness in ancient Greece.

Conclusions

My theme has been that one of the striking and distinctive features of much of early Greek thought, particularly when we contrast it with what we know from some other ancient civilisations, relates to the degree of overtness of innovation and of the contestability of tradition. The actual measure of free speech that the political situation secured in different city-states, at different junctures, over different types of political, moral, religious, and cosmological subjects, poses problems of great intricacy that cannot be explored here.**[187]** Yet in the grossest terms, there is certainly a gulf between Athens in the fifth century, even the Athens that prosecuted Anaxagoras and was to put Socrates to death,**[188]** and the Babylonia of Darius or the Egypt of Amasis.**[189]**

[186] I have taken my chief examples from early Greek medicine and pre-Socratic philosophy. But even though the authority of earlier writers plays an increasingly important role in parts of both science and philosophy from the late fourth century onwards, claims for originality are still often made. Aristotle himself offers a notable example at the end of the *Sophistici Elenchi (SE)* when he claims for his own studies in logic that they have initiated a new inquiry, 183a37ff., b34ff. Cf. also above, n. 102, with regard to Archimedes and Aristoxenus, and below, nn. 200, 206.

[187] The issues have been discussed by Momigliano 1973, Dover 1975, Finley 1975b, 1977, Lanza 1979, especially. On trials for impiety—where some of the stories generated concerning other philosophers show the influence of the fame of the model of Socrates—see especially Derenne 1930.

[188] The continuing difficulties that philosophers might encounter at Athens can be illustrated not just by the instance of Aristotle (see above, n.173) but also by the report in Diogenes Laertius 5.38 of the moves made against Theophrastus and others by Sophocles the son of Amphiclides, though in that case, according to the report, Sophocles was himself prosecuted, and after a year's exile Theophrastus returned.

Rivalry in claims to be wise starts almost as soon as we have any evidence to go on in Greece, and what counted as wisdom was an extraordinarily open-ended and negotiable question. Anyone could set himself up as a philosopher or as a sophist or, come to that, as a doctor. You depended not on legally recognised qualifications (there were none, we said, not even for doctors),**[190]** nor even simply on accreditation—though that was undeniably important.**[191]** What you had to rely on, largely, was your own wits and personality, and they were often judged by the verbal dexterity with which you presented your case, even when such verbal dexterity itself came to be suspect and so turned into a quality that had to be concealed to be fully effective.**[192]** Plato makes the sophist Gorgias say that he could take on and defeat any ordinary doctor in argument, whether in front of the Assembly (in

[189] We have, however, once again to discount some of the Greeks' own elaborations of the theme of the contrast between Greek freedom and Eastern tyranny in writers as diverse as the Hippocratic treatise *On Airs, Waters, Places* , e.g., 16, *CMG* 1.1.2.62.13ff., 23, *CMG* 1.1.2.78.3ff., and Plato, e.g., *Lg.* 694a–696b, even while this remains evidence of the Greeks' own attitudes.

[190] The lack of legal sanctions in relation to the practice of medicine in Greece is criticised in the Hippocratic *Lex* 1, *CMG* 1.1.7.5ff.; see Amundsen 1977, cf. Preiser 1970. On the legal position of doctors in Hellenistic times and later, see Nutton 1970, Kollesch 1974, Kudlien 1979.

[191] For example, although the doctrinal coherence of the doctors on Cos should not be exaggerated, and the extent to which the island offered more formal medical education than was available elsewhere is an open question, the evidence analysed by Cohn-Haft 1956 suggests that Coan doctors were particularly successful in obtaining appointments as public physicians in the fourth century and later. See also Sherwin-White 1978, chap. 7.

[192] It soon became a commonplace with public speakers of various kinds to disclaim any special skill in speaking themselves and to represent their opponents as dangerous and unscrupulous manipulators of argument: see, for example, Antiphon 5.1–7, Lysias 12.3, 86, 17.1, Isocrates 15.26, 42, and cf. Plato *Ap.* 17a–18a and the counterpoint in the exchanges between Socrates and Protagoras at *Prt.* 316d ff., 342a ff. See in general Dover 1974, pp. 25ff., de Romilly 1975, and cf. on Gorgias in particular, Segal 1962, cf. Rosenmeyer 1955, Verdenius 1981.

competition for a post as public physician) or, indeed, at the bedside**[193]** (where again we may remark the ease with which those two contexts are juxtaposed). No doubt Plato means his audience to see this as an exaggerated claim.**[194]** But in ancient Greece, where what passed for medical knowledge was both far less technical and more widely shared than now, the point was not an extravagant one; one of the elements of exaggeration, rather, we might say, is that, to judge from some of our Hippocratic texts, there were doctors who would have been well able to look after themselves in debate, even with a Gorgias. Even those who there appealed to what they represent as tradition, to the good old ways of medical practice, for example, argued to justify doing so. Tradition by itself, in many of the areas we are concerned with, at least,**[195]** carried little authority.**[196]** Pre-Socratic philosophers do not assert that earlier ideas should be accepted simply because of the prestige of those who had first proposed them; no more do Plato and Aristotle. Even those Hippocratic writers who saw the danger as one of an obsession with the newfangled do not base their case simply on appeals to authority figures.

In time, to be sure, the balance between these two, tradition and innovation, was to change very drastically, though I cannot here go into the stages, let alone discuss the possible underlying causes, of this

[193] Plato *Grg.* 456b–c, cf. also 452e, 459a–c, 514d ff. For other evidence of doctors called upon to address the Assembly, see Xenophon *Mem.* 4.2.5. At Plato *Plt.* 297e ff, 298c (cf. *Prt.* 319b f.) too, it becomes clear that the Athenians called in experts of various kinds including on medicine, though there the context is merely to take advice from them. Aristotle at *Rh.* 1403b32ff., 1404a12, draws a general contrast between the rhetorical style of delivery used in drama and in the Assemblies with that appropriate for teaching such a subject as geometry.

[194] In the continuation, *Grg.* 464d ff., Socrates draws a comparison with a competition between a cook and a doctor before an audience of boys.

[195] Here a contrast may be suggested with the role of appeals to the "ancestral constitution" at certain junctures in Athenian political debates: see Finley 1975a, chap. 2, 1983, p. 25.

[196] It is notable that according to *Vict.* 1.1 (L) 6.466.18–468.2, it takes the same intelligence to evaluate what has already been said correctly as to make original discoveries.

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complex process. We may simply note that from the end of the fourth century, increasingly, a series of great names—some, like Hippocrates, very

largely the constructs of the commentators**[197]** —came to be turned into just such authority figures, to whom appeals could be made as some kind of guarantee of the validity of the ideas associated with them. "Hippocrates," Plato, Aristotle, and later Ptolemy and Galen were transformed into such figures, and even though at an earlier period the written texts of Plato (for example) may well have helped Aristotle (for one) to develop and press home his *objections* to Plato's philosophy, the explicit aim of some of the late commentators was not to criticise those texts so much as to show how they contain the truth. Indeed, the sixth-century Aristotelian commentator Simplicius sought to show how Plato and Aristotle were in substantial agreement,**[198]** just as in the second century A.D. Galen already often aimed to reconcile Plato and Hippocrates.**[199]** One of the principal manifestations of that shift towards tradition**[200]** was, indeed, the turning of the written text into a vehicle for the transmission of authority rather than one for

[197] See, for example, Edelstein 1931, 1935, 1939/1967a, and especially the recent analysis of the growth and influence of the Hippocratic tradition in W. D. Smith 1979, especially chap. 3, pp. 177ff.

[198] See, for example, Blumenthal 1981; cf. Moraux 1984, pp. 441ff.

[199] This is the chief theme in the *De placitis Hippocratis et Platonis (PHP), CMG* 5.4.1.2 (K) 5.181.1ff., but the topic recurs.

[200] Yet innovativeness continued, of course, to be prized in many contexts, including in speculative thought. The very considerable power and originality of aspects of Hellenistic philosophy, for example, both dogmatic and anti-dogmatic, have only recently begun to be fully appreciated, thanks to such studies as those collected in Brunschwig, ed., 1978; Schofield, Burnyeat, and Barnes, edd., 1980; Barnes, Brunschwig, Burnyeat, and Schofield, edd., 1982; Schofield and Striker, edd., 1985. Moreover, as an explicit topic the desire to claim to be innovative can be illustrated in the second century A.D. in, for example, Lucian, who in the *Gallus* (22) 18 implies that it was from such a desire that the Pythagoreans introduced many of their more arcane rules and doctrines, while in that century or the next the obsession of Diogenes Laertius with the theme of first discoverers—along with the heterogeneity of the examples he cites—have already been remarked (n. 6 above). For further innovations both in the substantive theories, and the methodologies, of the exact and life sciences, see, for example, below, Chap. 3 pp. 163ff., Chap. 4, pp. 206ff., Chap. 5, pp. 230ff., 249ff.

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challenging it—thereby producing the opposite effect to the one Goody claimed for increasing literacy at an earlier period.**[201]**

Yet we should be careful not to suppose that the tendency to appeal to the authority of the past was uniform and all-pervasive in natural scientific inquiry, even in late antiquity. While when Galen cites Hippocrates it is almost always to agree with him,**[202]** the reverse is true of Galen's slightly older contemporary, Soranus. On nearly all the occasions when he cites Hippocrates or his followers in the *Gynaecology* it is to criticise them and to expose their mistakes.**[203]** Ptolemy, too, dissents from Hipparchus often enough, greatly though he admires him.**[204]** Nor should we underestimate the originality of Galen and Ptolemy themselves, for all their repeatedly expressed deference to the past. To say, as was once fashionable,**[205]** that they are just eclectic, is nonsense, though some of their own rhetoric tends to mislead in that direction. Their own contributions to their subjects, both as observers and as theorists, are of the highest order,**[206]** even when they present these as the elabora-

[201] Goody 1977, p. 37, does, however, allow that literacy also eventually encouraged what he calls the orthodoxy of the book.

[202] Galen does, however, on occasion refer to his own modest additions to what Hippocrates taught, especially in the domains of anatomy, e.g., *UP* 2.3 (H) 1.70.10ff., (K) 3.96.8ff., and of therapeutics, e.g., (K) 10.420.9ff., 425.6ff., 632.1ff., and cf. also with regard to pulse theory, *CMG* 5.8.1.134.1ff. ([K] 14.665.2ff.).

[203] See Soranus *Gynaecia (Gyn.)* 1.45, *CMG* 4.31.26ff., 3.29, *CMG* 4.112.14ff.; 4.13, *CMG* 4.144.2ff., 4.14–15, *CMG* 4.144.21ff., 145.14ff. In Celsus, too, Hippocrates is sometimes criticised for mistakes, e.g., *Med.* 3.4.12, *CML* 1.107.1ff., 6.6.1e, *CML* 1.260.3ff.

[204] See, for example, *Syntaxis* 3.1 (H) 1.194.3ff., 4.11 (H) 1.338.5ff., 5.19 (H) 1.450.11ff., 6.9 (H) 1.525.14ff.

[205] See, for example, Dampier-Whetham 1930, p. 53, on Ptolemy; Wightman 1950, pp. 330f., on Galen.

[206] Yet Ptolemy explicitly claims his theory of the moon's second anomaly and his solutions to the models of the five planets as his own: see *Syntaxis* 5.2 (H) 1.354.20ff., 9.2 (H) 2.210.8ff. Cf. also 4.9 (H) 1.327.16ff., 328.3ff., where he claims that the use of new methods enabled him to improve on his predecessors' work. The appeal to past authority has a more dominant role in key contexts in the *Tetrabiblos* (cf. above, Chap. 1 n. 152), which also provides a notable example of the prestige sometimes accorded to ancient manuscripts believed to contain esoteric learning; see *Tetr.* 1.21.49.14ff. That thecommentary form itself could provide the framework for a claim to originality can be illustrated, for example, from Porphyry, who justifies his undertaking the exegesis of Ptolemy's *Harmonics* in part on the grounds that no one had done this before him: *In Ptolemaei Harmonica* (*In Harm.*) 3.16ff. (Düring).

tion of the work of those of their predecessors of whom they most approve.

Down to the sixth century A.D. and even beyond, a Kuhnian tension is still a feature of parts of ancient speculative thought, even though the balance had shifted after the classical period from innovation towards tradition—to innovation mainly within, and represented as faithful to, the tradition, or rather to *one or another* of the plurality of rival traditions that still continued in most fields of investigation. What in some areas of thought was to alter the balance irrevocably—indeed by the sixth century A.D. had already done so in those areas—was the appeal to a particular text, the Bible, as revealed truth. The shift from reference to the "divine Hippocrates," the "divine Plato," and so on, to reference to the word of God may seem not so great in verbal terms, but it reflects fundamental differences not least in the underlying institutional realities: the creation of a church, the constitution of Christianity as the official religion of the empire, and the availability of a new battery of sanctions that could be deployed against the deviant. But those topics, too, are beyond the scope of our discussion here. What this study has attempted, rather, is to sketch out some of the problems presented by the balance of the tension at the very earliest stages of the Greek inquiry into nature. There in the classical period one crucial development was the opening up of the possibility, precisely, of development—if the oxymoron can be excused, the initiation of a tradition of, precisely, the contestability of tradition.

To conclude that the bias towards innovativeness characteristic of parts of early Greek speculative thought just confirms a Kuhnian verdict**[207]** that what we have here is, after all, not proper science—not "normal" science working within a dogmatic tradition or set of paradigms—is tempting and has an element of truth, but is one-sided and premature. On the matter of its one-sidedness, what that ver-

[207] See, most recently, Kuhn 1983, p. 567.

dict importantly leaves out of account is the stages through which proto-science itself passed. Fifth-century Greek speculative thought was no merely aberrant—rhetorical—interlude, intervening between tradition-oriented Egyptian and Babylonian medicine, mathematics, and astronomy, and again tradition-oriented Hellenistic science. The ancient Near Eastern evidence suggests some of the weaknesses, as well as some of the strengths, of the opposite bias towards conservatism—the negative effects, the constraints, of

monolithic authority. By contrast, the early Greek material we have reviewed illustrates not just the *excesses* to which egotism often led (though it does that) but also some of the positive aspects of aggressive innovativeness, in the canvassing of alternatives and the development of criticism through competition, as debate is opened up between rival theories and attention is focused on their grounds and articulation, indeed, on the question of the nature and foundations of science, medicine, and mathematics themselves. While too much attention paid to such second-order issues may detract from the business of getting on with the inquiries themselves, to pay no attention at all runs the risk of leaving the inquiries blind. A certain self-consciousness in the investigations and an awareness of alternatives, at least of rivals, were tolerably durable legacies bequeathed by early Greek to Hellenistic science, part of what then became, for some, revered tradition. For those early developments themselves to occur, however, what was needed was not just written texts, texts in which the figure of the author may not be visible against the background of the tradition, but (among other things) texts that through a strong authorial presence implied a personal accountability for the claims they contained.

And as to the matter of the prematurity of that judgement, our exploration of the Greek experience in the following chapters will provide the basis for the expression of certain other reservations and qualifications.

Chapter Three—
Dogmatism and Uncertainty

On several occasions already I have drawn attention to the elements of bluff and dogmatism in parts of early Greek science. Yet anti-dogmatic opinions are also prominent in other—sometimes even in the very same—works. A readiness to admit to not knowing the answers and to grant that you have been mistaken is still often thought part of the scientific, indeed a general intellectual, ideal. Examples where the ideal is put into practice can be given from modern science, although so too can cases where it has been ignored, and some writers would want to recommend that it should be ignored at least in certain circumstances.**[1]** We find what look like anticipations of those principles in some early Greek texts. The general question that this raises is, then, the interplay, or tension, between the dogmatic and the anti-dogmatic strains in Greek investigations into nature. In particular at what point, under what circumstances, with what motives and intentions did ancient scientists begin to acknowledge the possibility of their own mistakes?

As before, it is useful to establish a benchmark by the use of broad cross-cultural comparisons. First, scepticism about certain claims or claimants to special knowledge can be attested in many contexts in many peoples.

Shirokogoroff pointed this out in his classic study of the

[1] On the function of dogma in research see, for example, Kuhn 1963; cf. more generally in Kuhn 1962/1970 and the elaboration and modification of his position in Kuhn 1977.

Tungusi.**[2]** Evans-Pritchard stressed that the Azande often suspected particular witch-doctors of being frauds.**[3]** In his study of Ifa divination Bascom similarly noted that the honesty or knowledge of individual diviners may be questioned,**[4]** and Turner pointed out how attempts may be made to trip up individual Ndembu diviners.**[5]** The case of the Kwakiutl Quesalid, reported by Boas and popularised by Lévi-Strauss, is a poignant one.**[6]** Quesalid himself ended up as a shaman, but he had begun with the intention of showing that the ways of the local shamans were fraudulent, that their techniques were a set of tricks. What happened was that he tried other tricks that he learnt from other shamans from neighbouring groups and discovered that they worked: the sick reported remarkable recoveries, and Quesalid found himself, willynilly, a shaman. Again, in some mundane contexts, the recognition that there are limits to what any human being knows and can know is widespread and needs no illustration. It is a wise man who knows his own father, or, as Telemachus puts it, no one does.**[7]**

Our evidence from the ancient Near East is, once again, of exceptional value. Medicine, well represented in our extant texts, provides a particularly promising field of inquiry, since whether a disease has

[2] Shirokogoroff 1935, e.g., pp. 332ff., 389ff.

[3] See Evans-Pritchard 1937, p. 183: "Many people say that the great majority of witch-doctors are liars whose sole concern is to acquire wealth. I found that it was quite a normal belief among Azande that many of the practitioners are charlatans who make up any reply which they think will please their questioner, and whose sole inspiration is love of gain." But Evans-Pritchard went on to deny, p. 185, disbelief in witch-doctorhood in general.

[4] See Bascom 1969, p. 11: "The honesty or knowledge of individual babalawo may be questioned," though he went on: "but most are highly esteemed, and the system itself is rarely doubted." Cf. p. 70, where he notes that the blame for failures is shifted "from the system of divination to other causes, such as the ignorance or dishonesty of the diviner." Cf. Lienhardt 1961, p. 73, and more generally, and in connection with the ancient world, Jacques Vernant 1948 and the papers collected in J.-P. Vernant et al. 1974.

[5] V.W. Turner 1964, p. 242. Herodotus 1.46ff. (cf. 2.174), for example, provides Greek evidence for the testing of oracles.

[6] F. Boas 1930, pp. 1–41; cf. Lévi-Strauss 1958/1968, pp. 175ff.

[7] *Od* . 1.214ff. On various other occasions in Homer attention is drawn to certain limitations to human knowledge, e.g., *Il.* 2.484ff., *Od.* 10.190ff.

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been diagnosed correctly and whether the treatment adopted is the right one are questions of more than merely theoretical interest. Although, as we noted before,**[8]** the authors of Egyptian medical documents do not, as a general rule, intrude to vouch for their personal observations, reference is quite often made in general terms to experience. The Papyrus Ebers, for instance, on several occasions ends its account of a charm or remedy with the comment: "really excellent, [proved] many times."**[9]** Elsewhere the issue of the effectiveness of treatments is implicit. The relationship between the healer and the disease is frequently represented as a conflict, a hard-fought battle between them. In both Egyptian and Mesopotamian medicine, what causes the disease—the peccant material or force—is often apostrophised, commanded or cajoled to leave the patient, that departure being construed as a matter of negotiation.**[10]** Again, Egyptian, like later Greek, medicine explicitly recognised a category of complaints "where there is no treatment"**[11]** (though in practice in some such cases treatment is nevertheless attempted).

All of this goes to show that ancient Egyptian doctors, especially, were often aware of the limitations of their art and conscious of its difficulties. When claims for the effectiveness of remedies are made, they can, in principle, be controverted. Yet so far as our extant evidence goes, that mostly remained just a theoretical possibility. Neither Egyptian nor Mesopotamian medicine developed a tradition of the

[8] See above, Chap. 2, pp. 6f. and 63.

[9] See Ebbell 1937, pp. 29, 30, 42, 73.

[10] See, for example, Ebbell 1937, p. 105; Breasted 1930, p. 477; R. Campbell Thompson 1923–24, p. 31, 1925–26, p. 59. The general point remains, even though there are, to be sure, important differences within the diverse medical traditions in both Egypt and Mesopotamia.

[11] See, for example, Breasted 1930, cases 7, 8, 17, and 20; Ebbell 1937,

pp. 127f. The recognition of a category of cases that are hopeless and that cannot be treated can also be illustrated from the ethnographic reports: see, e.g., Shirokogoroff 1935, p. 334: "some shamans may refuse to attend cases which are known to be absolutely hopeless." Shirokogoroff further remarks, p. 385, on a case of a shaman who admitted to him and to a Manchu friend that he did not understand a situation, but that, from the report, appears to have been a private, not a public, admission of ignorance.

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criticism of current practice, any more than they did criticism of past custom and tradition themselves. In general, if doubts were felt about the efficacy of treatments or on the correctness of diagnoses, these were not usually expressed. Even when a case was deemed untreatable, this was generally asserted dogmatically.**[12]** Above all, there are no detailed records of particular failures of diagnosis or of cure (as opposed to mere expressions of despair), no debate between alternative treatments, let alone between rival schools of medicine with competing theories of disease.**[13]**

Dogmatism in Early Greek Natural Philosophy

One of the first things that strikes a student turning to the beginnings of Greek speculative thought, and first to pre-Socratic natural philosophy, is its dogmatism.**[14]** The wildest generalisations are offered with no suspicion that they may require qualification. True, this impression is partly one created by the doxographical sources on whom we often have to rely. They are concerned to record a sequence of positive theories ascribable to Thales, Anaximander, and the rest, uncomplicated by reservations or provisos.**[15]** Yet this impression is often confirmed when, as for several of the later pre-Socratics, we have more substantial evidence, in the form of original quotations.**[16]**

[12] As in the cases from Breasted 1930 cited in the previous note. Cf. J. A. Wilson 1952, p. 77.

[13] Cf., however, Bottéro 1974 who, in his study of divination in ancient Mesopotamia, notes (pp. 133f.) certain expressions of the difficulties encountered in particular problems in divination, and further draws attention (pp. 183ff.) to evidence that points to the development of different "schools" of omen interpretation, though without suggesting explicit debate between them.

[14] I discussed some aspects of this in G. E. R. Lloyd 1979, pp. 139ff. On other features of the issue of pre-Socratic dogmatism, compare Cornford 1952, chap. 3, with Matson 1954–55 and Vlastos 1955/1970 and 1975a, e.g., p. 87.

[15] This follows from the organisation of the material topic by topic in the doxographic tradition: see Diels 1879, cf. McDiarmid 1953/1970.

[16] These quotations themselves, however, have always to be related to thecontexts and concerns of those who report them, as has recently been emphasised by C. Osborne in her study of Hippolytus: C. Osborne forthcoming.

It is not as if there is much divergence, on this score, between otherwise radically divergent figures, such as Empedocles and Anaxagoras. Empedocles, for instance, announces categorically that bone consists of a certain definite proportion of the four "roots" or elements, earth, water, air, and fire.**[17]** Anaxagoras, who represents what is in many ways a quite different, Ionian, tradition of research, is sometimes just as positive in his assertions, for example, on the original state of the cosmos, when "all things were together" and "air and aether held all things,"**[18]** or on the production of earth from water and of stones from earth under the influence of cold.**[19]** Even those who were much later hailed as the forerunners of scepticism, such as Xenophanes and Democritus, were, on occasion, categorical enough.**[20]** Xenophanes certainly states that "there never was a man, nor will there ever be, who knows the certain truth about the gods and all the other things about which I speak" and that "seeming is wrought over all things."**[21]** But elsewhere he is prepared to speak of earth stretching down indefinitely below our feet, of the ocean as the begetter of the winds, and of our all being born from earth and water.**[22]** Democritus, too, though quoted as saying that we understand nothing exactly,

ἀτρεκές

, and know nothing truly,

ἐτεῇ

, about anything,**[21]** is also cited as confidently asserting nevertheless that atoms and the void alone are true or real,

ἐτεῇ

.**[24]**

[17] Fr. 96, cf. fr. 98, Aetius 5.22.1, Aristotle *De An.* 410a1ff., cf. 408a18ff., *PA* 642a18ff.

[18] Fr. 1, often quoted by Aristotle, e.g., *Ph.* 203a25, and Simplicius, e.g., *In Ph.* 155.23ff.

[19] Fr. 16.

[20] Sextus Empiricus is, indeed, often our source for earlier epistemological views that can be given a sceptical interpretation.

[21] Fr. 34 (quoted by many ancient writers; see Guthrie 1962, p. 395 n. 1). The difficulty of gaining knowledge of the gods is a topos that recurs, for example, in Protagoras fr. 4.

[22] Frr. 28, 30, 33 (with fr. 29).

[23] Frr. 6–10 and 117, on which see Sextus *M.* 7.135ff. especially. The most recent discussion of Democritus as a sceptic is that of Wardy forthcoming.

The Hippocratic Medical Writers

For more sustained expressions of doubt and uncertainty we have to turn to our other and more extensive main early source, the medical writers—not that they do not also provide examples of dogmatism to equal or surpass anything we find in pre-Socratic natural philosophy. On this, as on so many other topics, the positions adopted in our extant fifth- and fourth-century B.C. medical texts vary widely—and initially rather puzzlingly—from extreme dogmatism on the one hand to a self-conscious anti-dogmatism on the other.**[25]** How far, we may ask, are these apparently strongly contrasting attitudes to be correlated with different types of treatise, types of writer, types of audience, or a combination of some or all of these? In what respects are the attitudes in question indeed alternative and conflicting, or how far can we suggest a framework of explanation to cover both apparently opposed tendencies?

Dogmatism in the Hippocratic Corpus

We must begin with a fairly detailed review of the modalities and manifestations of dogmatism in the medical writers, since it is against that background that what I have called anti-dogmatism must be evaluated. The treatise *On the Art* , which we have considered before as an example of authorial egotism,**[26]** shows to what lengths some writers went to protect

themselves and the medical profession against any possible charge of incompetence or even of fallibility. Chapter 3 sets out what the author hopes to demonstrate, the word used being *apodeixis* . Medicine is first defined in terms of its aims, which include "the complete removal of the sufferings of the sick" and the "alleviation of the violences of diseases," and the writer claims that medicine achieves

[25] Some aspects of this problem have been discussed by Di Benedetto 1966, and by R. Joly 1966, pp. 240ff., 1980, pp. 287f.

[26] See above, Chap. 2 at nn. 45ff.

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these ends and "is ever capable of achieving them."**[27]** Against those who demolish the art of medicine by citing the misfortunes of those who die from their illnesses, he counters with a passage that is worth quoting at length:

> As if it is possible for doctors to give the wrong instructions but not possible for the sick to disobey their orders. And yet it is far more probable that the sick are not able to carry out the orders than that the doctors give wrong instructions. For the doctors come to a case healthy in both mind and body; they assess the present circumstances as well as past cases that were similarly disposed, so they are able to say how treatment led to cures then. But the patients receive their orders not knowing what they are suffering from, nor why they are suffering from it, nor what will succeed their present state, nor what usually happens in similar cases. . . . Which is then more likely? That people in such a condition will carry out the doctors' orders, or do something quite different from what they are told—or that the doctors, whose very different condition has been indicated, give the wrong orders? Is it not far more likely that the doctors give proper orders, but the patients probably are unable to obey and, by not obeying, incur their deaths—for which those who do not reason correctly ascribe the blame to the innocent while letting the guilty go free?**[28]**

Chapter 9 proceeds to distinguish between two main classes of diseases, a small group in which the signs are easily seen—where the disease is manifest to sight or to touch, for instance—and a larger one where they are not so clear. In the former group "in all cases the cures should be infallible, not because they are easy, but because they have been discovered."**[29]** So

far as the second group goes, "the art should not be at a loss in the case of the unclear diseases too."**[30]** The difficulty in achieving cures stems largely from delays in diagnosis, but this is more often due to the nature of the disease and to the patient than to the physician. The patients' own descriptions of their complaints are unreliable, for they have opinion rather than knowledge.**[31]** "For if they had understood [their diseases], they would not have incurred them. For it belongs to the same skill to know the causes of diseases and to understand how to treat them with all the treatments that prevent diseases from growing worse."**[32]** Again the writer's naive optimism comes out: the nature of our bodies is such that where a sickness admits of being seen, it admits of being healed.**[33]**

The breathtaking self-confidence of this treatise is far from unique. Drastically oversimplified pathological, therapeutic, and physiological doctrines—stated with apparently total self-assurance despite the manifest controversiality of the subjects in question—figure not just in

[31] *De arte* 11, *CMG* 1.1.16.23f.

[33] *De arte* 11, *CMG* 1.1.17.5f.

[34]

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other exhibition pieces, such as *On Breaths* ,**[34]** but also, for example, in *On Affections* ,**[35]***On Diseases* 1,**[36]***On the Sacred Disease* ,**[37]***On Fleshes* ,**[38]***On Regimens* 1,**[39]** and so on. *On the Places in Man* , for instance, is a work chiefly devoted to a quite detailed account first of certain anatomical topics and then of a range of morbid conditions and their treatments. Towards the end of the treatise as we have it**[40]** we find a chapter that announces: "The whole of medicine, thus constituted,

[35] *Aff.* 1 (L) 6.208.7ff.: "in men, all diseases are caused by bile and phlegm. Bile and phlegm give rise to diseases when they become too dry or too wet or too hot or too cold in the body."

[36] *Morb.* 1.2 (L) 6.142.13ff.: "all diseases come to be, as regards things inside the body, from bile and phlegm, and as regards external things, from exercise and wounds, from the hot being too hot, the cold too cold, the dry too dry, the wet too wet."

[38] The writer of *Carn.* sets out his version of a four-element theory in the opening two chapters as his own opinion, e.g., "it *seems to me* that what we

call hot is immortal" (2 [L] 8.584.9), "the ancients *seem to me* to have called this aither" ([L] 8.584.12, and cf. 5 [L] 8.590.5). Yet in the sequel there are few signs of tentativeness as he develops some highly speculative physiological and embryological theories about, for example, the interaction of the two principles he calls the glutinous and the fatty in the formation of the main viscera: see, e.g., 3 (L) 8.584.18ff., 4 (L) 8.588.14ff., and the claims to demonstrate in 9 (L) 8.596.9 and 16. Cf. also 1 (L) 8.584.5.

[39] *Vict.* 1.3 (L) 6.472.12ff., for example, states: "All the other animals and man are composed of two things, different in power, but complementary in their use, I mean fire and water."

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seems to me to have been discovered already. . . . He who understands medicine thus, waits for chance least of all, but would be successful with or without chance. The whole of medicine is well established and the finest of the theories it comprises appear to stand least in need of chance."**[41]**

On the Nature of Man , in particular, makes repeated claims to be able to demonstrate the theories it proposes.**[42]** While his opponents add to their speeches "evidences and proofs that amount to nothing,"**[43]** the author says that he will "produce evidences and declare the necessities through which each thing is increased or decreased in the body."**[44]** Yet his own positive evidences turn out to be very much of the same general type as theirs, even though their monistic conclusions are more extreme than his. He suggests that what influenced the monistic theorists he attacks was the observation that a certain substance may

[43] *Nat.Hom.* 1, *CMG* 1.1.3.164.14. His opponents in chap. 1 are monists who discourse about the nature of man beyond what is relevant to medicine and who claim that man is composed of air or water or fire or earth. In *Nat.Hom.* 2, *CMG* 1.1.3.166.12ff., he turns to attack monistic doctors who take blood, bile, or phlegm as the sole element of man. He has a general argument, against these, that if man were a unity he would never feel pain, since there would be nothing by which, being a unity, it could be hurt (*Nat.Hom.* 2, *CMG* 1.1.3.168.4f., with which compare Melissus fr. 7, para. 4). But against those who asserted that man consists of blood alone, for example, he demands that they should be able to show that there is a time of year or of human life when blood is obviously the sole constituent in the body (*Nat.Hom.* 2, *CMG* 1.1.3.168.9ff.).

[44] *Nat.Hom.* 2, *CMG* 1.1.3.170.6f.

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be purged from the body when a man dies. In some cases where a patient dies from an overdose of a purgative drug he vomits bile, in others maybe phlegm, and the monists, seeing this, then concluded that the human body consists of this one thing.**[45]** But while destructively the author sets about demolishing monism with powerful dialectical arguments, constructively when he seeks to establish that the body consists of the four humours, blood, phlegm, yellow bile, and black bile, his own chief argument too depends on the simple observation that all four are found in the excreta. This shows, to be sure, that all four are present in the body, but spectacularly fails to demonstrate that they are the elements of which it is composed.**[46]**

Alongside the frequent use of the vocabulary of evidence and proof, one of the key terms this author employs is necessity,

ἀνάγκη

, and its cognates, and the deployment of this word in this and other treatises offers an insight into their dogmatic character.**[47]** From the rich collection of uses in *On the Nature of Man* itself, the following may be cited. In chapter 3 he writes: "first, necessarily generation does not arise from a single thing: for how could one thing generate another unless it united with something?"**[48]** Later on he says that it is not likely that generation could take place from one thing, when it does not even occur from many unless those many are combined in the right proportions. He proceeds: "necessarily, then, since such is the nature of man and of everything else, man is not a single thing."**[49]** Further on in the

[45] *Nat.Hom.* 6, *CMG* 1.1.3.178.11–14.

[46] *Nat.Hom.* 5, *CMG* 1.1.3.176.10ff.; 6, *CMG* 1.1.3.180.2ff.; 7, *CMG* 1.1.3.182.12ff. In 5, *CMG* 1.1.3.178.5ff., he claims that the humours are congenital, on the grounds that they are present at every age and in both parents. Yet even if that were conceded, it would still not show that they are the chief, let alone that they are the sole elemental, constituents of the body.

[48] *Nat.Hom.* 3, *CMG* 1.1.3.170.8–9; cf. also 2, *CMG* 1.1.3.168.6.

[49] *Nat.Hom.* 3, *CMG* 1.1.3.172.2–3.

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same chapter we find: "necessarily, each thing returns again to its own nature when the body of the man dies, the wet to the wet, the dry to the dry, the hot to the hot, the cold to the cold."**[50]** Chapter 4 argues that when the humours in the body are well mixed and in the right proportion, the body is healthy, but that pain occurs when one of them is in excess or defect or is separated off from the others. "Necessarily, when one of them is separated and stands by itself, not only the place from which it has come becomes diseased, but also that where it collects and streams together causes pain and distress."**[51]** Again in chapter 5, having suggested that blood, bile, and phlegm differ to sight, to touch, in temperature, and in humidity, he goes on: "necessarily, then, since they are so different from one another in appearance and power, they cannot be one, if fire and water are not one."**[52]**

Clearly, logical and physical, conceptual and causal, necessity are not here differentiated. Many instances represent a conflation of one or more ideas that we might distinguish. Often the underlying idea seems merely to be the claim that something is always or usually the case. At the limit, the addition of the term *necessarily* appears to reflect little more than the writer's desire to assert his point with emphasis.

Similar uses of the term

ἀνάγκη

are common elsewhere in the Hippocratic Corpus, not only in the types of treatise that have provided most of our examples so far**[53]** but also in other major works,**[54]**

[50] *Nat.Hom.* 3, *CMG* 1.1.3.172.5–8.

[51] *Nat.Hom.* 4, *CMG* 1.1.3.174.3–6, cf. also 174.9f.

[52] *Nat.Hom.* 5, *CMG* 1.1.3.176.8–9. Cf. also 7, *CMG* 1.1.3.186.3; 8, *CMG* 1.1.3.186.17ff., and from after the main physiological section of the treatise (chaps. 1–8), e.g., *Nat.Hom.* 10, *CMG* 1.1.3.192.10; 12, *CMG* 1.1.3.198.5, 200.3 and 8.

[53] See, e.g., *De arte* 5, *CMG* 1.1.12.2 and 6; *Flat.* 7, *CMG* 1.1.95.7; 10, *CMG* 1.1.98.16; *Aff.* 37 (L) 6.246.20; *Morb.* 1.3 (L) 6.144.4, 17, 4 (L) 6.146.6, 9, 12, 13, 8 (L) 6.156.2, 4, 22 (L) 6.184.4, 186.10, 24 (L) 6.190.1, 7, 25 (L) 6.192.2; *Morb.Sacr.* 8 (L) 6.376.6, 13 (L) 6.386.7, 14 (L) 6.388.6ff., 17 (L) 6.392.19; *Carn.* 19 (L) 8.614.16; *Vict.* 1.4 (L) 6.474.15, 1.7 (L) 6.480.11, 1.9 (L) 6.484.4, 1.30 (L) 6.504.19, 1.36 (L) 6.524.7, 2.37 (L) 6.528.4; 2.38 (L) 6.530.14, 532.7, 2.40 (L) 6.538.4ff., 3.68 (L) 6.598.8, 3.71 (L) 6.610.9.

including some which, as we shall see later, are otherwise remarkable for their undogmatic or anti-dogmatic traits. Examples could be given from *Aphorisms* ,**[55]***On Ancient Medicine* ,**[56]***Wounds in the Head, On Joints* , and *On Fractures* .**[57]** The treatise *On Airs Waters Places* , too, frequently presents as matters of necessity the correlations it proposes

[56] See, for example, *VM* 22, *CMG* 1.1.54.6–10 ("as for what produces flatulence and colic, it belongs to the hollow and broad parts, such as the stomach and chest, to produce noise and rumbling. For when a part is not completely full so as to be at rest, but instead undergoes changes and movements, necessarily these produce noise and clear signs of movement"), and cf., e.g., *VM* 19, *CMG* 1.1.50.7ff., in the writer's general statement about causation (cf. below, Chap. 6 n. 14).

[57] In the surgical treatises, among the types of consequences and connections that are sometimes presented as matters of necessity are (1) the real or assumed consequences of lesions, (2) real or assumed anatomical facts and their consequences, and (3) the consequences of treatments, especially of faulty treatments. As examples of (1) we may cite *VC* 4 (L) 3.196.1f. (if the bone in the head is fractured when wounded, then necessarily contusion occurs), and *Art.* 63 (L) 4.272.14ff. (the doctor must bear in mind that in certain severe dislocations of the bones of the leg, when they project right through the ankle joint, the patient will necessarily be deformed and lame), and cf., e.g., *VC* 7 (L) 3.204.8f., 11 (L) 3.220.7f., 15 (L) 3.244.1ff.; *Art.* 13 (L) 4.116.23ff., 38 (L) 4.168.9f. As examples of (2): *Fract.* 3 (L) 3.424.10ff. (bending of a fractured arm necessarily causes a change in the position of the muscles and bones) and *Art.* 47 (L) 4.200.15ff. (in curvature of the spine one of the vertebrae necessarily appears to stand out more prominently than the rest) and cf.,e.g., *Fract.* 23 (L) 3.492.7ff. As an example of (3) we may cite *Fract.* 25 (L) 3.498.8ff., criticising bandaging that leaves the wound exposed ("the treatment, too, is itself evidence: for in a patient so bandaged the swelling necessarily arises in the wound itself, since if even healthy tissue were bandaged on this side and that, and a vacancy left in the middle, it would be especially at the vacant part that swelling and discoloration would occur. How then could a wound fail to be affected in this way? For it necessarily follows that the wound is discoloured with everted edges, and has a watery discharge devoid of pus"), and cf., e.g., *Art.* 14 (L) 4.122.16ff., *Fract.* 7 (L) 3.442.7ff., 16 (L) 3.476.11ff., 34 (L) 3.536.9ff.

between the aspect of a city and the character of its water, or between both

of those and the constitutions and endemic diseases of the inhabitants, or even between the political constitution and the character of the people. We may again illustrate very selectively from the rich fund of examples.

Thus we are told that in a city sheltered from the northerly winds but exposed to warm prevailing southerly ones, the water is "necessarily plentiful, brackish, surface water, warm in the summer and cold in the winter,"**[58]** while in a city that faces the risings of the sun, the water is "necessarily clear, sweet-smelling, soft, and pleasant,"**[59]** As for the effects of waters of different types, the writer states, for instance, that "stagnant, standing, marshy water is in summer necessarily warm, thick, and of an unpleasant smell, because it does not flow. But by continually being fed by the rains and evaporated by the sun it is necessarily discoloured, harmful, and productive of biliousness."**[60]** Dealing with physical constitutions and endemic diseases, the writer claims, for instance, that in northerly-facing cities that generally have hard, cold water, the inhabitants are "necessarily vigorous and lean."**[61]** Pleurisies and acute diseases are common, "for this is necessarily the case when bellies are hard."**[62]** Correlating the character and changes of the seasons with the diseases to be expected in them, the writer says:

[58] *Aër.* 3, *CMG* 1.1.2.26.23ff., 28.2f.

[59] *Aër.* 5, *CMG* 1.1.2.32.10ff., 13ff.

[60] *Aër.* 7, *CMG* 1.1.2.34.19–23, cf. also 36.25, 38.7f.; and 9, *CMG* 1.1.2.44.15f., 20f.

[61] *Aër.* 4, *CMG* 1.1.2.30.4.

[62] *Aër.* 4, *CMG* 1.1.2.30.8f., cf. 12f.; and 6, *CMG* 1.1.2.34.1f.

"If the winter be dry, with northerly winds prevailing and the spring wet, with southerly winds, the summer will necessarily be feverish and productive of ophthalmia."**[63]** Finally, correlating political constitutions and characters, the second half of the treatise suggests, for example, that "where men are ruled by kings, there necessarily they are most cowardly. . . . For their souls are enslaved and they are unwilling to run risks heedlessly for the sake of another's power."**[64]**

Even though other generalisations in this treatise are quite often explicitly qualified as holding only "for the most part" or just as being "likely,"

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,**[65]** the variety of connections claimed as being matters of "necessity" is, as these and many other examples demonstrate, considerable. Sometimes the grounds for the necessity are specified in a succeeding

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or because clause.**[66]** The point is important since it indicates at least an occasional recognition of the need, in principle, to support with evidence or argument the conclusions that are asserted with such emphasis; in that respect the dogmatists in the Hippocratic Corpus may be distinguished from even more extreme cases where no such recognition surfaces in the text at all. Yet it must also be remarked, first, that often no such grounds are adduced, and, secondly, that even when they are, they are often little more than cosmetic, and they generally fall far short of justifying the claims made as to the *necessity* of the conclusions.

[64] *Aër.* 23, *CMG* 1.1.2.78.3–5; cf. 16, *CMG* 1.1.2.62.20ff. Physiological and pathological correlations claimed as necessary also occur in the second part of the treatise, e.g., *Aër.* 19, *CMG* 1.1.2.68.15ff., and 24, *CMG* 1.1.2.80.3ff.

[65] See, for example, from the first part of the treatise, *Aër.* 3, *CMG* 1.1.2.28.5f.; 4, *CMG* 1.1.2.30.3, 7, 18, and from the second, *Aër.* 14, *CMG* 1.1.2.58.23; 24, *CMG* 1.1.2.78.15.

[66] As, for example, in the texts from *Aër.* 7, *CMG* 1.1.2.34.19ff.; 4, *CMG* 1.1.2.30.12f; and 10, *CMG* 1.1.2.46.22ff., quoted at notes 60, 62, and 63 above.

Uncertainty

In diagnosis and therapeutics, in pathology, anatomy, and physiology, the overwhelming impression created by a very considerable body of texts in a wide variety of Hippocratic works is one of their authors overstating their cases, representing as incontestable assertions for which their ground were —and must even have seemed to many of their own contemporaries to have been—tenuous or nonexistent. Yet that is only one side of the picture. Alongside the dogmatic tendencies I have illustrated—sometimes, indeed in the very same treatises—there are signs of tentativeness and caution, a readiness to admit to doubts and to mistakes, a recognition of the rashness of unsupported claims, explicit qualifications concerning how far a general

rule applies or about the limits of the writer's own firsthand knowledge, and statements insisting on the inexactness of the whole of medical practice.**[67]** In some cases, where, for example, the healer deliberately records his own errors, we are dealing with what appears to be—to judge from the extant remains of ancient medical literature, non-Greek as well as Greek—a quite unprecedented phenomenon.

We have noted before that criticisms of current medical practice are common in certain works,**[68]** but a critical attitude towards the mistakes of colleagues is of course quite compatible with and often accompanies overconfidence about the correctness of one's own ideas and procedures. In some Hippocratic texts, however, the author explicitly acknowledges that he was himself mistaken. Thus in *Epidemics* 5.27, which describes the case of one Autonomus who suffered from a wound in the head, the writer remarks: "It escaped my notice that he needed trepanning. The sutures which bore on themselves the lesion made by the weapon deceived my judgement, for afterwards it became

[68] See above, Chap. 2 at nn. 59ff. and n. 78.

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apparent."**[69]** The following chapter refers to the case of a young girl who was also wounded in the head, where trepanning was recognised to be indicated and was in fact carried out, but in this case, the writer says, not enough of the bone was removed.**[70]** The next two chapters describe two further cases where cauterisation was undertaken too late—in one case, we are told, thirty days too late—and both patients died.**[71]**

The author (or authors) of the surgical treatise *On Joints* not only describes some of his own mistakes but specifically notes that one such report is included so that others may learn from his own experience. Chapter 47 remarks on the difficulty of reducing humpback. "For my part . . . I know of no better or more correct modes of reduction than these. For straight-line extension on the spine itself, from below, at the so-called sacrum, gets no grip; from above, at the neck and head, it gets a grip indeed, but extension made here looks unseemly, and would also cause harm if carried to excess."**[72]** He then proceeds:

> I once tried to make extension with the patient on his back, and after putting an uninflated wineskin under the hump, then tried to blow air into the skin with a smith's bellows. But my attempt was not a success, for when I got the man well stretched, the skin collapsed, and air could

[70] *Epid.* 5.28 (L) 5.226.17ff.

[71] *Epid.* 5.29 (L) 5.228.5ff., and 30 (L) 5.228.10ff.

> not be forced into it; it also kept slipping round at any attempt to bring the patient's hump and the convexity of the blown-up skin forcibly together; while when I made no great extension of the patient, but got the skin well blown up, the man's back was hollowed as a whole rather than where it should have been. I relate this on purpose: for those things also give good instruction which after trial show themselves failures, and show why they failed.**[73]**

That the author and his colleagues were at a loss as to how to cure or even help a patient is often admitted in both the surgical works and the *Epidemics. Epidemics* 3 case 9 of the first series ends an account of a woman who suffered from an attack of ileus with the grim note: "it was impossible to do anything to help her; she died."**[74]** Case 5 of the second series remarks of a man who suffered from a sudden pain in the right thigh that "no treatment that he received did him any good."**[75]** Chapter 8 in the Constitution in this book comments more generally that there was little response to treatment and that purgatives did more harm than good,**[76]** and elsewhere writers in the *Epidemics* note that if

[74] *Epid.* 3 case 9 of the first series (L) 3.58.7f. Reporting of failures in clinical case-histories continues after the Hippocratic *Epidemics* . That Erasistratus' accounts of individual cases contained instances where the patient died is clear from the reports in Galen, e.g., (K) 11.200.1ff., 205.6ff., 206.5ff., 209.14ff., who exploits these failures for his own polemical purposes.

[75] *Epid.* 3 case 5 of the second series (L) 3.118.8.

the treatment had been different, a patient might have recovered or survived longer.**[77]**

On Joints , too, often refers to surgical cases where no remedy is possible**[78]** and repeatedly warns that the attempt to reduce certain intractable lesions does more harm than good.**[79]** Elsewhere the surgical writers explicitly say they do not know what to advise,**[80]** or withhold judgement.**[81]** The difficulties and dangers of treatment are mentioned also in other treatises, either in general terms, as in the famous first *Aphorism* ("life is short, art long, opportunity elusive, experience dangerous,

judgement difficult"),**[82]** or in relation to particular remedies, as, for example, the administration of hellebore or the practice of cautery or that of venesection.**[83]**

Many works draw attention to the incurability of certain diseases, though the advice they offer differs. Some suggest that the doctor should at least do what he can to help,**[84]** but others warn or instruct

[78] *Art.* 48 (L) 4.212.17ff., 63 (L) 4.270.7ff.

[79] *Art.* 63 (L) 4.268.12ff., 64 (L) 4.274.8ff., 65 (L) 4.274.20ff., 66 (L) 4.276.12ff., 67 (L) 4.278.5ff.; cf. *Fract.* 35 (L) 3.536.13ff.; *Mochl.* 33 (L) 4.374.16f., 376.2f.; *Aph.* 6.38 (L) 4.572.5ff.

[80] E.g., *Fract.* 16 (L) 3.474.17.

[81] E.g., *Art.* 1 (L) 4.78.2 ff., 80.13f., 53 (L) 4.232.12ff.; and frequently in *Epid.* , e.g., 1.4 (L) 2.626.3ff. Cf. *Praec.* 8, *CMG* 1.1.33.5ff., which advises the doctor, when in difficulties, not to hesitate to consult others.

[82] *Aph.* 1.1 (L) 4.458.1f.

[83] See, for example, *Aph.* 4.16 (L) 4.506.9f., 5.31 (L) 4.542.12f., 6.27 (L) 4.570.3f., 7.45 (L) 4.590.4ff.; *Acut.* 11 (L) 2.306.9ff., 308.7ff., 316.6ff.; *Art.* 40 (L) 4.172.5ff., 69 (L) 4.284.8ff.; *Fract.* 25 (L) 3.496.15ff., 30 (L) 3.518.4ff., 31 (L) 3.524.19ff.; *VC* 21 (L) 3.256.11ff. (on the hazards of trepanning), and cf. *Morb.* 1.6 (L) 6.150.6ff., which sets out a whole list of errors in judgement or practice the doctor should avoid.

[84] See, for example, *Art.* 58 (L) 4.252.8ff.

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him not to undertake such cases.**[85]***On Fractures* 36, dealing with fractures of the femur and humerus, illustrates the dilemma the doctor sometimes faced. "One should especially avoid such cases if one has a respectable excuse, for the favourable chances are few and the risks many. Besides, if a man does not reduce the fracture he will be thought unskillful, while if he does reduce it he will bring the patient nearer to death than to recovery."**[86]** Yet if some of the Hippocratic writers register their unease on this topic, it is important to note that none recommends that those patients whom they cannot or will not treat should have recourse to other modes of healing: none suggests that the sick should turn to the cult of Asclepius,**[87]** let alone try their luck with the itinerant sellers of charms

and purifications.

The theme of the inexactness of the medical art is a prominent one in several treatises and of particular interest for our inquiry. We shall be returning later to aspects of this in connection with the use of measurement.**[88]** Here we may simply note the recurrence of the motif in a variety of treatises. *On Ancient Medicine* , especially, develops the topic at some length. Exactness (

ἀκριβείη, τὸ ἀκριβές

, or

τὸ ἀτρεκές

) in the control of diet is difficult to achieve and small errors are bound to occur.**[89]** "I would heartily praise the physician who makes only small mistakes: exactness is rarely to be seen."**[90]** Up to a certain point the

[87] That *Morb.Sacr.* 1 (L) 6.362.10ff. is not to be taken in that sense (despite Herzog 1931, p. 149) is, I believe, clear: see G. E. R. Lloyd 1979, p. 48 n. 209. On occasion, however, *Vict.* 4 recommends prayer to the gods: see above, Chap. 1 n. 111; cf. n. 112.

[88] See below, Chap. 5 at nn. 134ff.; cf. also at n. 187.

[89] *VM* 9, *CMG* 1.1.41.18ff.: the correct diet cannot be determined by reference to some number or weight: the only criterion is bodily feeling. Cf. below, Chap. 5 n. 136.

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subject can be, and has been, made exact, but perfect exactness (

τὸ

ἀτρεκέστατον

) is unattainable. "But I assert that the ancient art of medicine should not be rejected as nonexistent or not well investigated because it has not attained exactness in every item. Much rather, since, as I think, it has been able to come close to perfect exactness by means of reasoning where before there was great

ignorance, its discoveries should be a matter of admiration, as well and truly the result of discovery and not of chance."**[91]**

Other treatises, too, develop similar themes. *On Diseases* I, which presents a highly dogmatic general theory of diseases based on bile and phlegm,**[92]** states nevertheless that there is, in medicine, no

ἀρχὴ

ἀποδεδειγμένη

, no demonstrated beginning or principle,**[93]** which is correct for the whole of the art of healing.**[94]** Discussing the

καιροί

, the turning-points of diseases which present the doctor with opportunities for intervention, the writer observes how much they differ from one disease to another and, after sketching out some of their variety, notes: "they have no exactness,

ἀκριβείη

, other than this."**[95]** Elsewhere too he stresses the differences between one body and another, one age and another, one illness and another, and repeats that "it is not possible to have exact knowledge,

τὸ ἀκριβὲς εἰδέναι

, nor to indicate at what

[92] See above, n. 36, on *Morb.* 1.2 (L) 6.142.13ff.

[94] *Morb.* 1.9 (L) 6.156.14ff.; with which compare *De arte* 4, *CMG* 1.1.11.5f.; *Carn.* 1 (L) 8.584.2ff.; and Diogenes of Apollonia fr. 1.

[95] *Morb.* 1.5 (L) 6.146.15ff., 148.15f.

time a patient will die, nor even whether this will be after a long or a short period."**[96]**

On the Places in Man , which asserts, as we saw, that the whole of medicine has been discovered already,**[97]** also observes that there is a good deal of variability in medicine**[98]** and remarks on the difficulties of determining the right moment for intervention.**[99]** Despite the ultradogmatic tone of the physiological and pathological theories in *On Regimen* ,**[100]** and despite the writer's claim that if one were present and could observe a man stripped and engaging in gymnastic exercises one could know just how to keep him healthy,**[101]** the third book of the treatise opens with a chapter that emphasises that it is not possible to set out exactly the proportions of food to exercise for men in general—because of their differences in constitution and age, and because of such other factors as the positions of cities, the changes in the winds, and the differences in foods, for example between one wheat and another.**[102]** No one, the writer says, has attained absolute exactness, though he claims he has got as close to this as is possible.**[103]** Even the sophistic piece *On the Art* , which makes, as we saw, extravagant claims for what medicine can achieve,**[104]** includes as part of its definition of medicine the refusal to treat cases "where the disease has al-

[97] See above, n. 41 on *Loc.Hom.* 46 (L) 6.342.4ff.

[98] *Loc.Hom.* 41 (L) 6.330.20ff.

[99] *Loc.Hom.* 44 (L) 6.338.6ff.

[100] See, for example, above, n. 39, on *Vict.* 1.3 (L) 6.472.12ff.

[102] *Vict.* 3.67 (L) 6.592.1ff.

[104] See above at nn. 27ff.

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ready won the mastery"**[105]** and ends by noting that it would not be fair to expect medicine to tackle the quite intractable diseases or to be unfailing in its remedies in those cases.**[106]**

The Purposes of Self-Criticism in the Hippocratic Writers

The material we have surveyed obviously presents difficult problems of interpretation. What are we to make of the different modes and degrees of dogmatism and tentativeness shown in different contexts and sometimes within one and the same treatise, the contrast between the apparently unhesitating self-confidence expressed in some texts and the caution and

doubt, the readiness to admit bafflement and error, in others? One possible suggestion might be that the contrast is to be associated with, and explained in part in terms of, the varying aims and audiences of the treatises in question.**[107]** Dogmatism and apparent self-confidence might be the stance adopted by those who addressed a lay public on general topics, whereas in treatises that represent the notebooks of working doctors and that were mainly directed to a professional audience of other practitioners, there would be a greater readiness to admit to hesitation or even to helplessness. Dogmatism would then be a tactic adopted in the context of a sophistic *epideixis* , often presented in a competitive situation where the winner was acclaimed by the audience of bystanders.**[108]** Confessions of uncertainty, on the other hand, would be limited to, or at least typical of, communications by and for practising medical men, and not for the general public.

[105] *De arte* 3, *CMG* 1.1.10.21f.

[106] *De arte* 13, *CMG* 1.1.19.4f., and cf. 8, *CMG* 1.1.14.1ff.

[107] The contrast between notebooks and treatises for public consumption is already a feature of ancient Hippocratic scholarship, though Galen, for instance, tends to refer to it, rather, to excuse certain loose expressions used in the notebooks: see, for example, *CMG* 5.10.2.2.19.5ff., 69.19ff., 75.25ff., 79.8, 80.16ff., 227.27ff., (K) 17A.822.16ff., 914.14ff., 922.3ff., 928.10f., 931.5ff., 17B.183.13ff.

[108] See above, Chap. 2, pp. 94ff. and n. 158, on *Nat.Hom.* 1, *CMG* 1.1.3.164.8ff., 166.2ff.

The importance of taking full account of the different audiences envisaged by the various types of writing extant in the Hippocratic Corpus needs no underlining. To elaborate some points from our discussion in Chapter 2: many suggestions that a medical man might make to his colleagues—and many ways in which he might wish to make them—would be totally inappropriate for a lay audience. This remains true, even though, as I stressed before, the lay/professional distinction was much less firm in ancient medicine than it is today, and there is ample evidence, from the fifth and fourth centuries, of an extended interest in medical topics—not just as a potential audience, but also as speakers and writers—among people who had no intention of actually engaging in medical practice. Plato would be one obvious example.**[109]**

Yet whatever features of this hypothesis we may wish eventually to retain, as stated it clearly will not do, for two main reasons. First we have seen that

there are treatises (including some that are reasonably well-defined unities, not multi-author concoctions) that combine a certain dogmatism at some points with an apparent tentativeness at others. By itself this would not be surprising, for it might simply reflect the varying degrees of difficulty of the topics dealt with and the varying degrees of confidence of the authors in dealing with them.**[110]** Yet to that, in turn, it must be said that in several of the cases we have considered, principally from *On Diseases* 1, *On the Places in Man, On Regimen* , and *On the Art* , it could not be claimed that dogmatism is confined to elementary or straightforward topics on which the authors might, with some justification, feel on safe ground. We have only to recall the claim in *On the Art* that for diseases with visible signs, "in

[109] The theory of diseases presented at *Ti.* 81e ff. was taken sufficiently seriously to be excerpted at length in the history of medicine in Anonymus Londinensis 14.11ff., 17.11ff. Other theorists there reported on might also be used to illustrate the point, for example, Philolaus 18.8ff., and Philistion 20.25ff., and we have mentioned before the non-specialist, general interest in medicine shown by some sophists.

[110] This may well be the more likely explanation in the case of some of the material from the surgical treatises that we considered.

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all cases the cures should be infallible" because they have been discovered. The *combination* of dogmatism and hesitancy in this and other works suggests a difficulty for any theory based on a clear-cut contrast between dogmatic treatises addressed to a general public and more cautious ones aimed at professional medical colleagues.

A second, more general objection to the hypothesis is that it is in danger of ignoring what most of the treatises we have considered have in common. Admittedly there are clearly identifiable differences between the two ends of what I referred to before as the *spectrum* represented by our extant texts—on the one hand the *epideixis* designed for public consumption, and on the other the almost exclusively technical notebooks. Yet there is a case for saying that, in their different ways and to different degrees, *both* types of production are exercises in persuasion.**[111]** That is obvious enough in the case of the sophistic *epideixis* . But even those writers who mainly had their fellow-practitioners in mind were also concerned to win their confidence, or at least to make sure that their own credentials were going to be recognised.

There is no reason to doubt the good faith of the author of the chapter in *On Joints* that sets out his own mistakes so that other practitioners may learn from them. At the same time we should not rule out the possibility that deliberate self-criticism may occasionally be motivated by a desire to suggest a mature experience in the art. Admittedly it seems paradoxical that

confessions of failure should be used in order to inspire confidence. Yet for a medical writer to demonstrate that he is well aware of the dangers of overconfidence would have a salutary effect. It would reassure prospective clients that they were dealing with a man who would not rashly undertake risky treatments nor raise hopes of cure unjustifiably. And it would help to persuade professional colleagues that the author was a man of experience conscious of the complexity and limitations of the art.

There is an important contrast here, not just between the more ten-

[111] Some aspects of the relationship between rhetoric and Hippocratic medicine are discussed in G. E. R. Lloyd 1979, pp. 88ff.; cf. Kudlien 1974.

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tative and the more dogmatic Hippocratic texts, but between the former and the claims for unqualified success that are characteristic of temple medicine. In the inscriptions set up in the shrine of Asclepius at Epidaurus it is 100 percent success that is recorded.**[112]** Some Hippocratic writers might well have wanted to dissociate themselves from the implicit claims to infallibility made in religious healing, even while other medical authors represented in the Corpus adopt a tone that rivals temple medicine in self-assurance.

The idea that self-criticism was sometimes deliberately deployed with such an intention cannot be confirmed directly. But it is perhaps suggestive that the main context in which an apparent tentativeness is expressed in certain treatises is in general remarks concerning the inexactness or variability of medicine, as in *On Regimen* 3 and *On Diseases* 1. It looks as if the explanation of these apparently mixed cases is neither that the authors are simply expressing a variety of attitudes on different topics, nor that they are merely inconsistent, nor yet that we are dealing with divergent material in composite works. Rather, it may be that even in otherwise dogmatic works, the inclusion of some indication of the inexactness of medicine had become, or was becoming, something of a convention or a commonplace.

If so, we should accept the apparent paradox. Dogmatism is clearly a stance frequently adopted to impress people, especially a lay audience, and especially on such questions as the origins of diseases in general or the constituents of the human body. Yet professions of uncertainty may also have a certain persuasive role, and while detailed accounts of failure in individual cases are confined to the more technical works that record actual clinical practice,**[113]** even more theoretical or philosophically oriented treatises occasionally include among their otherwise doctrinaire assertions a note to the effect that medi-

[112] See Herzog 1931.

[113] This is not just a matter of surgical practice, although surgery provides most of the more striking cases (cf. R. Joly 1980, pp. 287f.): some of the examples of recorded mistakes or faulty treatments in the *Epidemics* relate to general medicine, as for instance those at *Epid.* 5.18 (L) 5.218.2ff., and 5.31 (L) 5.228.14f., cited above in nn. 76f.

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cine is not certain. With some authors it becomes part of the definition of medicine, and of its claim to be the art that it is, that it is inexact. The recognition that it cannot do *everything* is sometimes used as a genuine warning, but it is also sometimes used to bolster claims (and they might be extravagant claims) that it *could* do a very great deal. That certain diseases are incurable is sometimes *not* taken as a sign of the inadequacy of the art in its current state but is turned into part of the medical man's knowledge,**[114]** part of what the medical man can be said to know.

Dogmatism and Uncertainty in the Fourth Century and Later

The continuing interactions of dogmatism and uncertainty have farreaching repercussions in many areas of Greek science long after the fifth century B.C. This is not just a matter of tone or style but relates to a deep-seated epistemological conflict where what are at stake are the answers to fundamental questions concerning the status of scientific theories and the possibility of science itself. With a wealth of material to draw on from philosophers of science, mathematicians, natural philosophers, and medical writers, our discussion must be even more drastically selective than ever.

Plato and Aristotle

We may begin with two central issues in the philosophies of science of Plato and Aristotle. When Plato comes to discuss the generation of the physical world, in the *Timaeus* , he refers to this repeatedly as a "likely story,"

εἰκὼς μῦθος

, but quite how we are to interpret this expression or evaluate the account we are given has been and continues to be much disputed.**[115]** Some suggestions that have been canvassed

[114] Cf. *De arte* 3, *CMG* 1.1.10.19ff. (accepting Heiberg's text).

[115] Among more recent discussions of the *Timaeus* in particular should be noted those of Witte 1964, Schulz 1966, Gadamer 1974/1980, Zeyl 1975, Vlastos 1975a, Scheffel 1976. On the general issue of the imperfection of per-ceptible phenomena, the contributions of Cooper 1970, Nehamas 1972–73, 1975, 1982–83, and Burnyeat 1976 are fundamental. Cf. Irwin 1977. For what follows see also G. E. R. Lloyd 1968a and 1983b.

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need not detain us long. The alternative expression,

εἰκὼς λόγος

, immediately shows that

μῦθος

here need not carry the connotation of fiction, over and above that of narrative account.**[116]** On the other hand, Taylor's claim that Plato was offering merely a provisional account**[117]** falls foul of the objection that an account of the physical world can, in Plato's view, under no circumstances be converted from a merely probable into a certain one.**[118]** Again, although Friedländer suggested that Heisenberg's uncertainty principle was in a sense anticipated by Plato,**[119]** it is as well to recognise where it differs from anything for which Plato's authority could be claimed. Two points are fundamental: first, the uncertainty principle is precise, in that it specifies that it is impossible to determine both the momentum and the location of a fundamental particle; secondly, it is grounded on reflections on the circumstances of experimental observation and intervention.

Both the nature of the reservations Plato expresses and their scope need to be considered carefully. The fundamental ontological distinction that dictates the status of any account of the physical world is, of course, that between being and becoming. What comes to be, insofar as it comes to be, cannot be the object of certain knowledge. That is stressed at *Ti.* 27d5ff. and repeatedly in what follows. Yet in respect of

[117] A. E. Taylor 1928, e.g., pp. 59ff.; criticised by Cornford 1937, pp. 29f.

[118] Comparisons with the hypotheses of modern science are, then, liable to be misleading, at least insofar as they had better not be, in principle, beyond the reach of empirical support or refutation.

[119] Friedländer 1958–69, vol. 1, p. 251. Heisenberg himself occasionally referred in admiring terms to Plato's atomic theory, e.g., 1945/1952, p. 57,

1955/1958, pp. 59f.; cf. also Feyerabend 1981b, p. 84.

being itself no such reservation applies; on the contrary, concerning what is stable Timaeus makes the considerable demand that the accounts should "so far as possible" be irrefutable and unchangeable (or invincible) ones.**[120]** Whenever the cosmologist or the natural philosopher has to do with the intelligible model—the Forms—after which the visible cosmos is constructed, there should, in principle, be no falling short.**[121]**

Moreover, the claim in respect to the particular cosmological account set out in the *Timaeus* is that it is "inferior to none in likelihood."**[122]** The visible cosmos is not of course identical with the intelligible model. In the work of creation the Craftsman has to bring order into what is already in chaotic motion.**[123]** He has to contend with the factor Plato calls necessity or the wandering cause.**[124]** Yet he made the cosmos as like the model as he could. Four points are worth emphasising. First, the model the Craftsman uses is itself eternal and unchanging; the importance of this is spelled out at *Ti.* 28a ff., where the inferiority of any production based on a created model is stressed. Secondly, the product of his workmanship is *good* . The theme is a recurrent one and is given a triumphant climax in the final sentence of the *Timaeus* , where the likeness of the intelligible model is described as a perceptible god, greatest and best and fairest and most perfect.**[125]**

[123] *Ti.* 30a, 52d ff.

[124] *Ti.* 47e ff. Broadly, reason "persuades" necessity in the sense that the best ends are secured within the framework of the possibilities set by the inherent properties and characteristics of the material available.

Thirdly, what the Craftsman does is to bring order into precosmic chaos or disorder, an order that the natural philosopher, for his part, can and should study and discover.**[126]** Fourthly, the Craftsman's own work is indissoluble, even if that of the lesser divine Craftsmen does not share that characteristic.**[127]**

While the whole account of becoming is undercut as no more than a likely story, the particular grounds for hesitation expressed concerning particular items in the exposition vary. Timaeus baulks at a detailed discussion of planetary motion, for instance, in part for fear of making his account disproportionately long.**[128]** Length is again a factor mentioned when he draws back on the question of why the half-equilateral is the fairest of scalene triangles.**[129]** Here we are told that if someone is able to give a better account of the construction of the elementary bodies, his is the victory of a friend, not an enemy.**[130]** The longer account that Timaeus refers to, but does not give, would not necessarily be an end of the matter: "should anyone refute this and discover that it is not so, we do not grudge him the prize."**[131]** But that is certainly not to deny, but, rather, to assert, that the problem might be advanced.

To be sure, elsewhere the deflation of the value and importance of parts, at least, of the exercise is underlined, as, for example, at *Ti.* 59c–d when Timaeus speaks of probable accounts of becoming as a "moderate and intelligent pastime" undertaken "for the sake of recreation,"**[132]** where the particular problems he is about to tackle concern the varieties and compounds of the simple bodies. Again at 68b–d we are told that to try to state the different proportions of the constituents that go to make up particular colours would be to betray an ignorance

[127] *Ti.* 41a–b.

[128] See *Ti.* 38d–e, and cf. 40c–d, which refers to the pointlessness of a discussion of planetary motion without visible models to consult.

[129] *Ti.* 54a–b.

[130] *Ti.* 54a.

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of the difference between God's nature and man's, for on that question not even a probable account is possible.**[133]**

To take stock on the chief issues that concern us here, the first and most obvious contrast between Plato and most of his predecessors relates to the explicitness with which he confronts the question of the status of any account of the natural world. Secondly, on many topics on which both the pre-Socratic natural philosophers and many medical theorists had tended to express themselves dogmatically, implicitly making unqualified claims for the correctness of their assertions, Plato states his reservations, both general and particular, about the possibility of attaining certainty. But if in principle

and in practice he is undeniably an anti-dogmatist on many questions in physics and cosmology, we should not underestimate the dogmatic elements that remain. If certainty is not possible concerning becoming, it is demanded "so far as possible" for being, including the intelligible order the divine Craftsman uses as his model. Above all, the issue of the goodness of the created world and of its creator is not a matter of doubt. In the *Timaeus* we are merely told that it would be impious to deny this,**[134]** but in the *Laws* Plato was to treat those who denied that the world is the product of benevolent, rational order as a threat to the state he there describes and, as such, subject to sanctions of formidable severity, including death, if they do not modify their views.**[135]** Teleology especially is not negotiable.

By insisting that physics deals with what is true "for the most part"**[136]** as well as with what is true "always" Aristotle drew a distinc-

[133] *Ti.* 68b and 68d. When, in the latter text, Timaeus says that no test is possible for us, since god alone is able to mix the many into one and again to dissolve the one into the many, whereas no man is or ever will be capable of doing either of these, the blending in question appears to be not a matter of mixing pigments, but one of combining fundamental atomic particles. Yet the expression is unclear, and Plato certainly does not distinguish as carefully as he might between the two types of blending, leaving it possible to read Timaeus' remarks as suggesting that one cannot even hope to discover which pigments added to which give which compound colours.

[134] *Ti.* 29a.

[135] See *Lg.* 889a ff., 896d, 897b–c, 907d ff., and cf. also *Phlb.* 28d–e.

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tion that marks his distance from Plato. Yet that very distinction points to a well-known crux in his thought, one that relates, broadly, to the tension between the demand for scientific theories to be certain and an appreciation that not everything in science is or can be. On the one hand Aristotle insists, in the *Posterior Analytics* , that

ἐπιστήμη

must fulfil some very tough conditions indeed. Understanding**[137]** is of what cannot be otherwise than it is, and demonstrative understanding in particular depends on premises that are true, immediate, better known than, prior to, and explanatory of the conclusions.**[138]** On the other hand, the study of nature is not limited to what is true "always" but includes also what is the case "for the most part." In practice, in his scientific treatises syllogisms are rare, and demonstrative

ones fulfilling the criteria set out in the *Posterior Analytics* rarer still. At the same time, reminders of the provisional nature of the results, and of the need for further investigation, are frequent.**[139]**

Yet—an obvious question—how can a study that deals with what is true only "for the most part" conceivably be a science or yield *episteme* as Aristotle defines it? At least, if "for the most part" is interpreted in a statistical sense (more than half, but not all), then syllogisms that have both premises true "for the most part" will not yield conclusions that hold "for the most part," let alone universally. If most B's are A, and most C's are B, it will not follow that most C's are A; it will not follow that any are. Moreover, when only one premise is true for the most part, the other universally, they will not necessarily combine to give a conclusion that is true "for the most part." "Most B's are A" and "all C's are B" together do not yield "most C's are A." And when the

[138] See *APo.* 1.2.71b20ff., and 1.4.73a21ff.

[139] The most famous examples of this come in his discussion of the reproduction of bees, where he states, among other things, that the facts, or what occurs, have not been sufficiently ascertained (*De Generatione Animalium, GA* , 760b27ff.), and on the question of the number of celestial spheres needed to account for the motions of the sun, moon, and planets (*Metaph.* 1073b10ff., 13ff.; cf. 1074a14ff.). Cf., e.g., *Somn. Vig.* 454b21ff., *Resp.* 476a5ff., *GA* 721a1f., 14ff., 741a34ff., 746b4ff., 757b22f., 762a33ff.

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major premise is universal, the conclusion is again not "for the most part" if that is taken to exclude "universally": "all B's are A" and "most C's are B" together do not rule out "*all* C's are A."

In the light of the difficulties in Aristotle's opaque and elliptical discussions**[140]** it has been suggested that "for the most part" is not purely statistical but is used, rather, as a temporal operator (i.e., "not always") or as a quasi-modal operator ("not necessarily") or corresponds to some admittedly unanalysed notion of what holds "by nature."**[141]** Yet Aristotle himself, it must be said, nowhere elucidates the concept, nor does he explain how syllogisms incorporating propositions true "for the most part" meet the requirements laid down for understanding in the opening chapters of the *Posterior Analytics* , notably the requirement that it is of what cannot be otherwise than it is.

Some alleviation of the general problem is possible. The *Posterior Analytics* , it has been argued,**[142]** has primarily a pedagogic aim: it presents certain recommendations about how a mature science is to be taught, or at least

about how to set out a body of theorems in good deductive order so that their connections are revealed and the explanations they incorporate are grasped as the explanations they are. Manifestly, Aristotle has very little to say, in this work, on the problems of discovery, about how scientific understanding is acquired in the first

[140] The chief texts in the *Organon* are in *APr.* 1.27, *APo.* 1.30 and 2.12. In *APo.* 1.30.87b19ff., Aristotle remarks that when, in syllogisms, the propositions are necessary, the conclusion is also necessary; when for the most part, the conclusion is also likewise—where "for the most part" is clearly contrasted with "necessary," 87b22–23—but the main aim of the chapter is to refute the notion that there is demonstrative knowledge of what happens by chance. At *APo.* 2.12.96a8–19, he stipulates that for the conclusion to be true "for the most part," as opposed to universally, the middle term must also hold "for the most part"—where this is contrasted with what holds universally, for all and always (96a15–16). However, the greatest difficulty for the statistical view is in *APr.* 1.27.43b33ff. There when the "problems" are "for the most part," the syllogisms consist of propositions that are—either all or some of them—"for the most part," and this appears to envisage the possibility of syllogisms with *both* premises true "for the most part."

[141] Apart from the perceptive remarks in J. Barnes' commentary on *APo.* , 1975 ad loc., see the full discussion in the elegant paper devoted to the topic by Mignucci 1981.

[142] Most forcefully, in recent times, by J. Barnes 1969/1975.

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place.**[143]** At the same time, the examples he gives show that his discussion is not restricted to the already well-established disciplines such as mathematics and the exact sciences. Although most of his illustrations are drawn from such fields, a fair number, particularly in the second book, relate to zoological or botanical questions.**[144]** Presumably he has in mind an ideal that these studies can *eventually* attain, for certainly they had not done so in his day.**[145]** Yet for that ideal to be realised, *either* we have to imagine that the studies as set out will deal solely with universal and necessary propositions, *or* the difficulties in extending the schema to cover propositions true only "for the most part" have to be resolved—with corresponding modifications, no doubt, to the ideal itself.**[146]**

The value of the model in the *Posterior Analytics* as a *model* of demonstration, however, remains. If we recall the complex and confused uses of the terms for necessity and demonstration in the Hippocratic writers, we can see the advances made.**[147]** Aristotle stipulates precisely

[144] See, for example, *APo.* 98a35ff., 99a23ff., b4ff.; cf. J. Barnes 1969/1975, pp. 70ff.

[145] This is not to deny that connections can be found between the recommendations of the *Posterior Analytics* and the actual practice of the zoological treatises. Lennox 1987, for instance, has recently drawn attention to the concern, in the latter, to establish the widest class of which a character is true (cf. *APo.* 1.4.73b26ff. and 5.74a4ff.): cf. also Pellegrin 1986. But neither of these studies tackles the problems raised by physics dealing with what is true "for the most part."

[146] At *APo.* 94a36ff., in his discussion of the different types of causes that may serve as middle terms, Aristotle even gives an example of a historical explanation to illustrate the efficient cause. Moreover, this is one that involves reference to a singular term (the Athenians' raid on Sardis, cited as provoking the Persian war) and so falls outside the scope of the theory of the syllogism set out in the *Prior Analytics* .

[147] See above at nn. 47–66. Aristotle himself notes at *PA* 639b21ff., cf. *Metaph.* 1015a20ff., both that many of his predecessors reduced their explanations to the necessary (by which he means that they took no account of the final cause) and that they failed to distinguish the senses of necessity. This is not to deny, of course, that certain distinctions continue to be ignored by Aristotle himself, as is clearly shown by Sorabji, 1980a, and cf. Waterlow 1982b.

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what conditions have to be met to justify the claim that conclusions have been demonstrated. True premises and valid inference are not enough: the premises must be prior to and explanatory of the conclusions. In a sequence of demonstrations the ultimate starting-points (they comprise definitions, axioms, and hypotheses) must themselves be indemonstrable (on pain of an infinite regress) but known to be true.**[148]** Whatever other obscurities remain, necessity as logical consequence is now deployed with confidence, and we have a whole subtle discussion of necessity as a modal operator, even though, again, the precise interpretation of many points in Aristotle's treatment remains controversial.**[149]**

But the clarity of the model has been bought at a price in terms of the range of its applicability. In mathematics and the exact sciences there is little difficulty in fulfilling Aristotle's criteria: a body of theorems can be presented in systematic order and their derivation from a set of axioms and definitions made clear. Yet the situation is very different in the natural sciences, and not just for the reason already mentioned, that these deal with propositions some of which are true only "for the most part." For the model to be applicable here we have also to be able to answer the thorny question of the

nature of the indemonstrable starting-points. Over and above the general regulative principles that govern all discourse—the laws of contradiction and of excluded middle—what will count as axioms in zoology and botany, in meteorology or geology?**[150]** Can we envisage the definitions in such fields having the status of such starting-points?

[148] See *APo.* 72a5ff., cf. b18ff., 76a31ff.

[149] See especially Sorabji 1980a, and cf. Lear 1980, chap. 1.

[150] The dictum that "nature does nothing in vain" is often appealed to, in the zoological treatises especially, as the grounds for particular explanations, and it may be said to act as some kind of general regulative principle governing the zoologist's inquiry, one which must be accepted for that inquiry to be fruitful and one that is chiefly to be justified by the results obtained by its use. On the other hand, it is unlike both the laws of excluded middle and contradic-tion, and the particular mathematical axioms that Aristotle mentions (such as the equality axiom that if equals are taken from equals, equals remain: e.g., *APo.* 76a41). No attempt is or can be made to prove the latter, while the former are to be supported by what he calls an "elenctic demonstration" (*Metaph.* 1006a11ff., 15ff.), which proceeds by pressing any opponent who would deny them to signify something, to himself or to another (cf. Lear 1980, pp. 98ff.). Clearly, opposition to the dictum that nature does nothing in vain cannot be dealt with in *that* way. Rather, we have several serious attempts to discuss the consequences of its denial, notably in *Ph.* 2.8.198b10ff. and *PA* 1.1, especially 640a18ff., even if in the body of the physical treatises it is thereafter generally assumed—as Aristotle may hold it has to be, for progress to be made in scientific inquiry.

Definitions and demonstrations are, as Aristotle points out in his acute if often problematic discussions of their interrelations in the *Posterior Analytics* ,**[151]** crucially interdependent. Take first one of his astronomical examples. Lunar eclipse is not just any loss of light that the moon suffers (a cloud obscuring it will not count), but loss of light due to the interposition of the earth. But if you ask for the explanation, you will receive the information packed into the full definition. Why does it suffer eclipse? Because the earth intervenes.**[152]** Similarly, in one of the botanical examples alluded to:**[153]** deciduousness is not just any

[151] See *APo.* 75b30ff., 2.8, 93a14ff., 10, 93b29ff., 94a11ff. Aristotle recognises that before we are in a position to give a definitive definition we sometimes have some grasp of the subject inquired into (93a21ff., 29ff.), as well as some understanding of the meaning of the term (93b29ff.), though these points do not receive much elaboration in his discussion. See R. Bolton 1976, Ackrill 1981.

loss of leaf that a tree suffers (if it is diseased and drops its leaves, that will not count), but loss of leaf from the coagulation of the fluid or sap at the junction of the leaf-stalk. The fully fledged syllogism that sets out why broad-leaved trees are deciduous might run: all trees that have sap that coagulates are deciduous; all broad-leaved trees have sap that coagulates; so all broad-leaved trees are deciduous. This syllogism meets the criterion for a demonstration, for the conclusion is drawn through a middle term that is explanatory. But everything depends, evidently, on the truth of the definition.

From this point of view, all that the theory of demonstration does is to provide a set of rules for the proper articulation of propositions in such a way as to reveal the explanations as the explanations they are. But for the botanist in the field, obviously the chief task is the acquisition of the knowledge, not its articulation. As Aristotle's own practice in, for example, the zoological treatises shows, he is generally far short of being able to resolve the main problems he raises by demonstrations containing explanations proceeding from incontrovertible starting-points.

But whatever tensions remain between his theory and his practice, Aristotle did, as we said, transform the understanding of demonstration, and whatever the limitations of his model in other fields, the possibility of its application to mathematics and the exact sciences was clear. His formal logic differs in several fundamental respects from Euclid's practice in the systematic presentation of a body of geometrical theorems in the *Elements.* **[154]** In particular, Euclid's arguments are not syllogistic, and attempts to recast them in syllogistic form reveal the artificiality of that exercise. **[155]** Yet what Aristotle's theory and Euclid's practice have in common is the conception of demonstration proceeding by rigorous deductive argument from indemonstrable

[154] The problem of the relationship between Euclid's postulates and Aristotle's axioms has been much discussed, e.g., by Scholz 1930/1975, H. D. P. Lee 1935, Einarson 1936, von Fritz 1955/1971, Berka 1963, I. Mueller 1969, Gómez-Lobo 1976–77, Hintikka 1981, Leszl 1981.

[155] See, for example, Corcoran 1973, R. Smith 1978, Novak 1978, and especially I. Mueller 1969, 1974, and 1981.

axiomatic starting-points. Here, one may say, was a new style of wisdom indeed. Moreover, the ideal thus jointly derived (very roughly speaking) from Aristotle and Euclid was to prove enormously influential**[156]** and well beyond the exact sciences.**[157]** Physical scientists and medical writers too often advocated demonstration *more geometrico* and in some unlikely contexts. Just what will count as the indemonstrable premises in element theory and meteorology, in physiology, embryology, and pathology, a difficulty already in Aristotle, does not become much clearer later in those such as Galen who also hankered after deductive certainty.**[158]**

[156] Although the incontrovertibility of mathematical arguments was their pride, the insistence on rigorous deductive proof had certain inhibiting consequences within Greek mathematics itself. The best-known and most obvious illustration of this comes with Archimedes' comments on his own mechanical method, based on a notion of indivisibles, in his *Method* , (HS) 2.428.18ff., 438.16ff. In this a plane figure whose area is to be determined is thought of as composed of a set of parallel lines indefinitely close together, balanced against corresponding lines of the same length in a figure of known area: thus the desired area can be found in terms of the known one. Archimedes remarks explicitly that this is not a method of proof, only one of discovery: its results have thereafter to be proved strictly, using reductio and the method of exhaustion. The method remained unexploited by later Greek mathematicians, in part, no doubt, because this treatise itself was not generally known. Yet that is not the whole story, since some of the theorems in *On the Quadrature of the Parabola* implicitly depend on a similar method. An additional, more substantial factor may lie in the reluctance on the part of Greek mathematicians of any period to rely on informal methods. It is in this respect that the contrast between Greek mathematics and the mathematics of Cavalieri and others in the seventeenth century is most marked. On the Archimedean method, see, for example, Knorr 1981 and 1982a, who argues that the difficulty lay with its application of mechanical ideas rather than with its use of indivisibles, and who remarks, 1981, pp. 174ff., on the inhibiting influence of the insistence on formal procedures.

[157] Even in the exact sciences in antiquity, however, axiomatisation is generally incomplete judged by modern standards: cf. further below, Chap. 5 n. 111.

[158] Galen's own treatise *On Demonstration* , in fifteen books, is not extant, but even while he recognises the stochastic elements in medicine, references to the ideal of geometrical method and attempts to deploy it recur throughout his work. There is now a very full discussion of this aspect of Galen's methodology in J. Barnes (forthcoming). For the fragments of the work *On Demonstration* see I. von Müller 1897.

Nevertheless, one strand of a dogmatic tradition thereby attained a measure of philosophical respectability in the wake of the development of the theory of demonstration and of its practice in the mathematical sciences, and one clear benefit from this was a greater awareness of the questions of the formal analysis and validity of arguments—though Stoic logic takes as much of the credit for this as Aristotle's.**[159]** Yet over against that tradition, the recognition of the dangers of dogmatism, and a certain tentativeness and open-mindedness, can also be amply exemplified in some of Aristotle's successors, as they can in Aristotle himself. We may turn first to Theophrastus and to two works in the Aristotelian Corpus that are in the main the products of the Lyceum—the *Problemata* and the *Mechanics* —for excellent illustrations of the continuing tension between the dogmatic and the tentative.

Theophrastus

In a wide variety of contexts Theophrastus engages in a far-reaching examination of many of the fundamental assumptions on which natural scientific inquiry had been based, including in particular many Aristotelian positions, though in his criticism of these Theophrastus often elaborates points to which Aristotle himself had drawn attention. The short treatise *Metaphysics* , for instance, mainly consists of a review of difficulties—and certainly not just in Aristotle. Thus although Theophrastus accepts Aristotle's notion that the ultimate source of movement in the universe must itself be an *un* moved mover that acts as an object of desire, the nature of the impulse it imparts requires, he says, more discussion. The heavenly bodies so moved are a plurality, and their motions are complex and opposed to one another.

> For if that which imparts movement is one, it is strange that it does not move all the bodies with the same motion; and if [alternatively] that

[159] See M. Frede 1974. The fact that Galen, who is in general no friend of the Stoics, uses Stoic propositional logic freely is good evidence of its wide-spread influence: see, for instance, the examples commented on by Furley and Wilkie 1984, pp. 53, 258f., 265f.

> which imparts movement is different for each moving body and the sources of movement are more than one, then their harmony as they move in the direction of the best desire is by no means obvious. And the matter of the number of the spheres demands a fuller discussion of the reason for it; for the astronomers' account is not adequate. It is hard to see, too, how it can be that, though the heavenly bodies have a natural desire, they pursue not rest but motion.**[160]**

Developing points that were in most cases anticipated by Aristotle himself,**[161]** Theophrastus later raises questions concerning the limits of teleological explanation. "With regard to the view that all things are for the sake of an end and nothing is in vain," he says, "the assignation of ends is in general not easy . . . , and in particular some things are difficult because they do not seem to be for the sake of an end but to occur, some of them, by coincidence, and others, by some necessity, as in the case both of celestial and of most terrestrial things."**[162]** What purpose, he asks, do changes in sea level serve, or breasts in male animals? Some things—his example is outsize horns in deer—are even

[161] Lennox 1985 and Vallance forthcoming now provide careful studies of Theophrastus' critique of earlier views on the issue of teleology in *Metaph.* chap. 9. In the final analysis, as Vallance argues, Theophrastus' own position has more in common with that of Aristotle than with extreme positions on either side of him, that is, with either the out-and-out anti-teleologists, on the one hand, or, on the other, those who failed to recognise any limits at all to teleological explanation (which Aristotle certainly did). (Those explicitly named in this chapter include Speusippus, Plato, and the Pythagoreans, 11a23, 27.) At the same time Theophrastus focuses critically on some examples found in Aristotle (such as the position of the windpipe, 11a9ff.; cf. Aristotle *PA* 665a9–26) and is concerned to spell out more explicitly than Aristotle had done that teleological explanation is not applicable in many cases.

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harmful to the animals that possess them.**[163]** There is even a certain plausibility in the view that many things come about spontaneously and "by the rotation of the universe."**[164]** "If they have no purpose, we must set certain limits to the final cause and to the tendency towards what is best, and not assume it absolutely in every case. . . . For even if this is the desire of nature, it is clear that there is much that does not obey nor receive the good."**[165]** He is confident in rejecting the view that good is rare and that evil predominates in the universe, but he ends his catalogue of problems with: "but at any rate these are the questions we must inquire into."**[166]**

A similar searchingly aporetic tone characterises his discussion not just of high-level metaphysical and methodological issues, but also of several particular physical problems. Take, for example, his treatment of the nature of fire. In the treatise devoted to that question he raises a series of difficulties connected with the idea that fire is a simple body, like earth, water, or air. "Of the simple bodies," he begins, "the nature of fire has the most special powers."**[167]** None of the other simple bodies can generate itself, but fire can do so. Most of the ways it comes to be, whether natural or artificial, appear to involve force. Even if that is not the case (he corrects himself) yet "at least this much is clear: fire has many modes of coming-to-be, none of which belong to the other simple bodies."**[168]** The most

important difference, he proceeds, is that the other simple bodies are self-subsistent and do not require a substratum, whereas fire does, "at least so far as is clear to our percep-

[163] *Metaph.* 10a28ff., b11ff. That the reference to "incursions" and "refluxes" of the sea at 10a28ff. is more probably one to changes in the general level of the sea, rather than to tides, has recently been argued by Vallance forthcoming.

[164] *Metaph.* 10b26ff.

[166] *Metaph.* 11b24.

[168] *Ign.* 1.3.1ff., 2.3.12ff.

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tion."**[169]** "In sum, everything that burns is always as it were in a process of coming-to-be, like movement,**[170]** and so it perishes, in a way, as it comes to be and as soon as what is combustible is lacking it too itself perishes."**[171]** "Hence it seems absurd to call this a primary [substance] and as it were a principle, if it cannot exist without matter"—that is, the fuel.**[172]**

By the end of the treatise he has exposed many of the weaknesses in common Greek assumptions about fire and has questioned the too easy assimilation of fire to the other so-called simple bodies. Yet he has clearly not abandoned that notion entirely. His dilemma is evident: he recognises many of the fundamental difficulties; he realises that many issues require further investigation and his parting remark, at the end of the work,**[173]** is to promise a more exact discussion of some topics on another occasion. Yet he has no *new* constructive element theory to propose, nor does he answer the question of the nature of fire that he set himself, beyond stressing the diversity of its forms and examining some of these.

A second instructive example that illustrates both his acute perception of weaknesses in widespread assumptions and also some of the difficulties he experienced in pressing home his critique comes from his botany, from his discussion of spontaneous generation. This is mentioned in the *Inquiry concerning Plants* as the first of the ways in which plants and trees may come to be.**[174]** In the *Causes* he begins his

[173] *Ign.* 76.51.3f.

discussion: "Spontaneous generation, broadly speaking, takes place in smaller plants, especially in those that are annuals and herbaceous. But still it occasionally occurs too in larger plants whenever there is rainy weather or some peculiar condition of air or soil. . . . Many believe that animals also come into being in the same way."**[175]** Yet having thus apparently endorsed the common view, he goes on to introduce reservations:

> But if, in truth, the air also supplies seeds, picking them up and carrying them about, as Anaxagoras says, then this fact is much more likely to be the explanation. . . . Moreover, rivers and the gathering together and breaking forth of waters purvey seed from everywhere. . . . Such growths would not appear spontaneous, but, rather, as sown or planted. Of the sterile sorts, one might, rather, expect them to be spontaneous, as they are neither planted nor grown from seed, and if they come to be in neither way, they must necessarily be spontaneous. But this may possibly not be true, at least for the larger plants; it may be, rather, that all the stages of development of their seeds escape our observation, just as was said in the *Inquiry* about willow and elm. Indeed, the development of seed escapes observation also in many of the smaller herbaceous plants, as we said about thyme and others, whose seeds are not evident to the eye, but evident in their effect, since the plant is produced by sowing the flowers. Further, in trees too some seeds are hard to see and small in size, as in the cypress. For here the seed is not the entire ball-shaped fruit, but

> the thin and unsubstantial bran-like flake produced within it. It is these that flutter away when the balls split open. This is why an experienced person is needed to gather it, who has the ability to observe the proper season and recognize the seed itself.**[176]**

In many cases, therefore, propagation comes from unnoticed seed. The succession of trees in wild forests and in the mountains could not easily be maintained by spontaneous generation. "Instead there are two alternatives: to come from a root or from seed."**[177]** He notes that woodcutters report that among trees of the same kind some individual specimens are sterile. There is still a possibility that their seed passes unnoticed; alternatively, the trees become sterile because all their nourishment is used up on other parts. But if this can happen in individuals or kinds that can and do bear fruit, it may not be impossible for the same thing to happen in whole kinds. He concludes: "Let this be given merely as our opinion; more accurate investigation must be made of the subject and the matter of spontaneous

generation must be thoroughly inquired into. To sum the matter up generally: this phenomenon necessarily occurs when the earth is thoroughly warmed and when the

collected mixture is changed by the sun, as we see also in the case of animals."**[178]**

Once again, despite that "necessarily" in the final sentence, the tentativeness of his discussion, and his recognition of the difficulties of the subject, are clear. He is conscious of the need for more research, and his emphasis on the point is no mere lip-service to a theoretical ideal, but a plea for the continuation of his own work made in the realisation that it had already brought tangible results in the investigation of particular cases. Nevertheless, despite his demonstration that many instances of what had been taken to be spontaneous generation were not such, he ends by reaffirming his belief that the phenomenon must occur. This might look like a failure of nerve, but again the dilemma he faced is plain. To have asserted that there was seed in *every* case of believed spontaneous generation would, after all, have been to go well beyond the evidence available to him.

Although he takes over substantial sections of Aristotelian physics, the aporetic and anti-dogmatic tendencies in Theophrastus are surely impressive. Like Aristotle he often calls for further research, and like Aristotle he does so with the voice of extensive experience, not just in botany but in other fields. His challenge to accepted assumptions is no mere bluff, even if he ultimately endorses some of the theories that he subjects to blistering attack. Yet *not* to abandon those theories was surely right in the main, at least until a superior alternative could be proposed. Rather, his exposure of the weaknesses of many key doctrines, combined with his tenacity in retaining them, illustrates the difficulty of suggesting such alternatives and the impasse in which even sustained critical inquiry found itself in many areas of physics in the fourth century B.C.**[179]**

Question-Posing in the Peripatetic Tradition

The question-posing style of discussion is widely developed in other works emanating from the Lyceum, whether or not this reflects the direct influence of Aristotle and Theophrastus themselves. The extant *Problemata* is not authentic, though we know that Aristotle wrote a treatise of that

name.**[180]** The work we have in the Aristotelian Corpus consists of thirty-eight books of "problems" not just on natural philosophical topics ranging from mathematics and music theory to medicine and biology, but also on questions of character and ethical issues. The collection as a whole displays a highly developed, even obsessive, curiosity, even if this is often directed at trivial issues or problems with little prospect of resolution—as when, in the book on justice, for instance, the writer puzzles over why wealth is more often found in the hands of the wicked than the good,**[181]** or when, in the book on sympathetic action, he asks why yawning is caused by the sight of others yawning.**[182]** Elsewhere, however, the problems are sometimes more suggestive, as when such questions are raised as why the ears of divers burst,**[183]** or why substances kept in closely covered vessels remain free from putrefaction,**[184]** or why the plague alone of diseases infects especially those who associate with the patients.**[185]**

[180] See Flashar 1962, pp. 316ff., 356ff. Parts of the *Problemata* we have evidently draw directly on, or may simply record, Theophrastus' accounts of similar problems: see, for example, Regenbogen 1940, cols. 1559f.; Müri 1953, pp. 21ff.

[181] *Pr.* 29.8.950b36ff.

[182] *Pr.* 7.6.887a4ff.

[183] *Pr.* 32.2.960b8ff.

[184] *Pr.* 25.17.939b12ff.

[185] *Pr.* 1.7.859b15ff; cf. 7.8.887a22ff., on which see Nutton 1983.

Each chapter begins with a "why?" question, and the answers proposed often take the form of a further question: is it that so and so,

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. . .? We should not, however, exaggerate the extent to which this approach reflects any genuine tentativeness about the answer. Just as it appears that certain expressions of uncertainty become conventionalised in some Hippocratic texts,**[186]** so too to propose a physical explanation in the form of a question could be no more than a matter of presentation.**[187]** A sequence of problems in book 1 appears to be derived from the Hippocratic treatise *On Airs Waters Places* . Although that work is not mentioned, the writer of the *Problemata* appears to take

as his starting-point propositions that had been asserted dogmatically in it. Chapter 10 of the Hippocratic treatise has been quoted already: "If the winter be dry, with northerly winds prevailing, and the spring wet, with southerly winds, the summer will necessarily be feverish and productive of ophthalmia."**[188]** This supposed fact figures in *Problemata* 1.8: "Why is that, when north winds have been prevalent in the winter, if the spring is wet, with southerly winds, the summer is unhealthy with fever and ophthalmia?"**[189]** The Hippocratic writer had gone on: "For when stifling heat succeeds while the ground is still wet with the spring rains and southerly winds, the heat will necessarily be twice as great."**[190]** The *Problemata* chapter does not use the term

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for necessity,**[191]** and the explanation is introduced in the form of a question: "is it because . . . ?" But the explanation is an adaptation from the medical writer, notably in its reference to "stifling heat,"

[186] See above, p. 134.

[187] The much later scholastic use of the method of *quaestiones et solutiones* shows, if it needs showing, how discussions that begin with an air of tentativeness may take on a dogmatic character in the outcome.

[188] *Aër.* 10, *CMG* 1.1.2.46.22ff.; see above, n. 63, and cf. *Aph.* 3.11 (L) 4.490.2ff.

[190] *Aër.* 10, *CMG* 1.1.2.46.24ff.

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,**[192]** and the proposer shows no signs of not being totally confident that he has resolved his problem satisfactorily.**[193]**

Yet it would be a mistake to dismiss the whole question-posing approach as *mere* window-dressing, a superficial veneer masking what are essentially dogmatic attitudes. First, there are occasions when genuinely alternative answers are on offer. That so often none of those canvassed is very promising does not detract from this as evidence of a willingness to consider alternatives.**[194]** Secondly and more importantly, on some problems it is recognised that no finally satisfactory answer is forthcoming and the writer admits to being left with some at least of his original puzzlement.

Two of the most notable instances come not from the *Problemata* but from the *Mechanics* .**[195]** This too adopts a question-posing approach to the rather diverse mechanical problems it raises.**[196]** Chapter 33, for instance, wrestles with the question: "How is it that a body is carried along by a motion not its own, if that which started it does not keep following and pushing it along?"**[197]** Here the solution offered gets no further than a version of the idea that the impelling force continues to act via the medium.**[198]** Chapter 32 is more remarkable still, in that it ends in self-confessed failure: "Why is it that objects that are thrown

[192] In the plural at *Pr.* 860a3 and 4.

[193] Other texts in *Pr.* 1 may also be compared with *Aër* . Thus with *Pr.* 1.9.860a12ff., cf. *Aër.* 10, *CMG* 1.1.2.48.13ff.; with *Pr.* 1.10.860a35ff., cf. *Aër.* 10, *CMG* 1.1.2.50.21ff.; with *Pr.* 1.11.860b8ff., cf. *Aër.* 10, *CMG* 1.1.2.52.2ff.; with *Pr.* 1.12.860b15ff., cf. *Aër.* 10, *CMG* 1.1.2.52.4ff.

[194] See, for example, *Pr.* 1.13.860b26ff., 2.4.866b28ff.; *Mechanica (Mech.)* 34.858a23ff., 35.858b4ff.

[195] The *Mechanics* is generally thought to have been written by a member of the Lyceum, and some have favoured Strato's authorship.

[196] Thus 6.851a38ff. considers why it is that the higher the yardarm is raised, the quicker the vessel travels with the same sail and in the same breeze, and 21.854a16ff. asks why it is that dentists extract teeth more easily with tooth-extractors than with their bare hands.

[198] Cf. also Aristotle's hesitant discussions of this problem at *Ph.* 215a14ff. and 266b27ff., and cf. Furley 1976, p. 94.

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eventually come to a standstill? Do they stop when the force which started them fails? Or because of being drawn in a contrary direction? Or is it due to the downward tendency, which is stronger than the force which threw them? Or is it absurd to discuss such questions, while the principle escapes us?"**[199]**

Schools of Medical Thought: Dogmatism, Empiricism, Methodism

The texts we have considered illustrate some of the tensions between the

dogmatic and the tentative, the speculative and the self-restrained, in post-Aristotelian natural philosophy. But in Hellenistic medicine, varieties of dogmatism and scepticism or anti-dogmatism are elevated into self-conscious methodologies. The so-called Dogmatic medical school

(δογματικοί or λογικοί)

[200] takes its origin from the objections of its opponents. Those labelled Dogmatists in our sources (they include Herophilus and Erasistratus and, often, Hippocrates himself) would not have recognised themselves as forming a distinct sect with shared principles and practices. But first the Empiricists—beginning perhaps with Philinus of Cos around the middle of the third century B.C. —and then also the Methodists—followers of Themison (first

century B.C.) and of Thessalus (first century A.D.)**[201]** —set themselves apart from those of their predecessors and contemporaries whom they represented as having certain methodological principles in common.

The evidence we have to rely on is in many cases indirect and much of it comes from critical or hostile sources. Empiricism, especially, is poorly represented by original texts,**[202]** and so too is Methodism until we come to Soranus in the second century A.D. Neither Celsus nor, more obviously, Galen is an impartial witness, and aspects of their reports are suspect as historical accounts.**[203]** On the other hand, both are, obviously, evidence for the *currency* of certain ideas at the time they wrote,**[204]** and we can analyse their interpretations of the debate even if we have to bear in mind that they are *their* interpretations and even if the evidence to confirm or refute what they attribute to some of the contending parties is often not available.

Celsus presents a particularly full picture of the alternatives as he saw them in the proem to the first book of his *De medicina* .**[205]** The chief issues, as he reports them, relate to the aims, limits, and methods of the medical art. Those grouped together as Dogmatists are represented as holding that medicine should investigate not only (1) the so-called evident causes (such as heat and cold considered as causes), but also (2) hidden or obscure ones, as well as (3) natural actions (such as breathing and digestion, in other words, physiology) and, finally, (4) internal anatomy.**[206]**

Of these four inquiries the Empiricists are said to accept only the first, that into evident causes, alone. The other three are not just superfluous but impossible, since "nature cannot be comprehended"; the

[201] Whether Themison and Thessalus are to be considered the founders or the forerunners of Methodism is disputed: see, for example, Edelstein 1935/1967a.

[202] The evidence is collected in Deichgräber 1930/1965.

[203] The point is given particular emphasis in Rubinstein 1985.

[204] Celsus wrote in the first century A.D. , Galen in the second.

[205] *Med.* 1 pr. 12ff., *CML* 1.19.4ff. Celsus sets out his own position in the dispute at 1 pr. 45ff., *CML* 1.24.24ff.; see Mudry 1982.

[206] *Med.* 1 pr. 13, *CML* 1.19.11ff.

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doctor's task is to treat individual cases and for this purpose he must be guided by the manifest symptoms of the patient alone. Against the Dogmatists, the Empiricists rejected "reasoning" and accepted "experience" alone as the criterion. It is this that has suggested cures; it is from experience that medicine has been built up and on which it must continue to rely. It is not a discovery made following reasoning; rather, the discovery came first and the reason for it was sought afterwards. Moreover, where reasoning teaches the same as experience, it is unnecessary, and where different, it is opposed to experience and should be rejected.**[207]**

As Celsus makes the Empiricist argue:

> It does not matter what produces the disease, but what relieves it. Nor does it matter how digestion takes place, but what is best digested—whether concoction comes about from this cause or that, and whether the process is concoction or merely digestion.**[208]** We have no need to inquire in what way we breathe, but what relieves laboured breathing; nor what may move the blood-vessels, but what the various kinds of movements signify. All this is to be learnt through experiences. In all theorising over a subject it is possible to argue on either side, and so cleverness and fluency may get the best of it. However it is not by eloquence, but by remedies, that diseases are treated. A man of few words who learns by practice to discern well would make an altogether better practitioner than he who, unpractised, overcultivates his tongue.**[209]**

[207] *Med.* 1 pr. 27f., 36, *CML* 1.22.1ff., 23.4ff. According to Celsus, the Empiricist response to the possibility, entertained by the Dogmatists, that new diseases may arise was still to insist that the practitioner should not attempt to theorise about causes, but see to which existing disease the condition is similar and try out remedies that had proved successful in such other similar cases.

[208] This appears to allude to the long-standing debate on the nature of digestion, where Herophilus, following Aristotle, argues that it involves "concoction," while Erasistratus and the Erasistrateans explain the process in purely mechanical terms, as the result of the trituration or pounding that the food is subjected to in the stomach before being absorbed, as chyle, into the blood-vessels communicating with the liver.

[209] *Med.* 1 pr. 38–39, *CML* 1.23.16–27: *sed has latentium rerum coniecturas ad rem non pertinere, quia non intersit, quid morbum faciat, sed quid tollat; neque ad rem pertineat, quomodo, sed quid optime digeratur, siue hacde causa concoctio incidat siue illa, et siue concoctio sit illa siue tantum digestio. Neque quaerendum esse quomodo spiremus, sed quid grauem et tardum spiritum expediat; neque quid uenas moueat, sed quid quaeque motus genera significent. Haec autem cognosci experimentis. Et in omnibus eiusmodi cogitationibus utramque partem disseri posse; itaque ingenium et facundiam uincere, morbos autem non eloquentia sed remediis curari. Quae si quis elinguis usu discreta bene norit, hunc aliquanto maiorem medicum futurum, quam si sine usu linguam suam excoluerit* .

Again, "even students of philosophy would have become the greatest medical practitioners, if reasoning could have made them so. But as it is, they have words in plenty, but no knowledge of healing at all."**[210]**

The third main medical group, the Methodists, had their own subtle and often rather maligned ideas about treatment,**[211]** but on the essential topic we are concerned with here they are represented by both Celsus and Galen as agreeing with many of the criticisms that the Empiricists brought against the Dogmatists, for example, about their theorising about hidden causes.**[212]** While Celsus reports the Empiricists as *asserting* that nature cannot be comprehended,**[213]** Sextus makes it

[210] *Med.* 1 pr. 29, *CML* 1.22.11–13: *Etiam sapientiae studiosos maximos medicos esse, si ratiocinatio hoc faceret: nunc illis uerba superesse, deesse medendi scientiam* . The rejection of the idea that medicine can be learnt from books has a long history. The Hippocratic surgical treatise *Art* . 33 (L) 4.148.13ff. refers to the difficulties of explaining surgical procedures, in particular, in writing, and cf. Plato *Phdr.* 268c, Aristotle *EN* 1181b2ff.

[211] The Methodist idea of the three common conditions, the constricted, the lax, and the mixed, came under particular attack. To the chagrin of Galen, especially, (e.g., *Sect.Intr.* 6, *Scr.Min.* [H] 3.15.2ff., [K] 1.83.1ff.), the Methodists were reputed to have claimed that medicine could be learnt in six months: cf. M. Frede 1982. The three common conditions were neither themselves disease entities nor causes of diseases, but generalisations about the state of the body that guided the practitioner in deciding upon treatment (seen as a matter of counteracting the lax with the constricted and vice versa). As we can see from Soranus (see G. E. R. Lloyd 1983a, pp. 182ff.) and from Caelius Aurelianus, not only in principle but also in practice Methodist pathology and therapeutics stayed a good deal closer to what was directly observable than rival theories and were a good deal simpler than they were.

[212] See Celsus *Med.* 1 pr. 57, *CML* 1.26.27f., cf. Galen *Sect.Intr.* 6, *Scr.Min.* (H) 3.13.21ff., 7.17.3ff., 18.1ff., (K) 1.81.6ff., 85.14ff., 86.17ff.

[213] *Med.* 1 pr. 27, *CML* 1.22.4, where Celsus makes this the grounds, for the Empiricists, of the claim that such an inquiry is superfluous (*superuacuam*); cf. Sextus *P.* 1.236.

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appear that the Methodists withheld judgement on that issue.**[214]** The inquiry into the obscure is to be rejected because it is useless, rather than (as the Empiricists are said to have held) impossible.**[215]** If that is correct, then the distinction between these two medical groups would in certain respects be analogous to that between some of the Academic sceptics and such Pyrrhonian sceptics as Sextus Empiricus himself, in that the former *asserted* that the nonevident cannot be grasped (and so in that respect were negative Dogmatists) while the latter withheld judgement on that issue.**[216]**

Both Empiricists and Methodists thus appear to have combined in a withering attack on the speculative tendencies that had, in fact, been highly developed in Greek medicine from the first—even while other early texts, as we saw, emphasised the difficulty and tentativeness of medicine, resisting the ambition to treat it as an exact science and representing it as a conjectural or stochastic art.**[217]** But faced with the inordinate array of pathological theories, based on humours, opposites, elements, the supposed disorders of the pneuma, the supposed blocking of the pores in the body, and so on,**[218]** both Empiricists and Methodists may have agreed in concentrating on the *practical* aims of medicine. The great strength of their positions, as these are reported, lay in

[215] See Galen *Sect.Intr. 6, Scr.Min.* (H) 3.14.14ff., (K) 1.82.6ff., and cf. Celsus *Med.* 1 pr. 57, *CML* 1.26.26ff.

[216] Cf. Edelstein 1935/1967a, pp. 186f. The interpretation of the position of the Academic sceptics is, however, much disputed: see, for example, Couissin 1929/1983, Striker 1974, 1980, Sedley 1983b, Burnyeat 1984.

[218] The disputes between rival positive pathological theories begun by the Hippocratic writers continue with Herophilus, Erasistratus, and Asclepiades, among many others. For Herophilus, see von Staden forthcoming; for Erasistratus, see especially Lonie 1964 and cf. Longrigg 1981. My colleague John Vallance is preparing a comprehensive edition of Asclepiades.

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both cases in the focus on what had proved to be successful in experience, even though our secondary sources press the difficulties of the rejection of "reasoning"**[219]** and even though, no doubt, the interpretation of experience itself, and of the "appearances" to be relied on, may well have been more problematic than either group allowed.

Yet if the anti-dogmatic and anti-speculative tendencies in Greek medicine thereby reach their apotheosis, there was a price to pay. Both Empiricists and Methodists are said to have ruled out dissection and vivisection. In the latter case, human vivisection, as practised, according to Celsus,**[220]** by Herophilus and Erasistratus on criminals "received out of prison from the kings," was repudiated by most people, including Celsus, with disgust**[221]**—though Celsus mentions a Dogmatist justification in terms of the balance of advantage: the benefits accruing to "multitudes of innocent men of all future ages" justified the sacrifice of

[219] M. Frede 1982 has, however, recently argued strongly for the possibility, within Methodism, of the deployment of reason and even of theoretical beliefs, provided these are recognised as speculative.

[220] *Med.* 1 pr. 23–24, *CML* 1.21.15–21, which reports a Dogmatist view: *longeque optime fecisse Herophilum et Erasistratum, qui nocentes homines a regibus ex carcere acceptos uiuos inciderint, considerarintque etiamnum spiritu remanente ea, quae natura ante clausisset, eorumque positum, colorem, figuram, magnitudinem, ordinem, duritiem, mollitiem, leuorem, contactum, processus deinde singulorum et recessus, et siue quid inseritur alteri, siue quid partem alterius in se recipit* .

("Herophilus and Erasistratus proceeded in by far the best way: they cut open living men—criminals they obtained out of prison from the kings—and they observed, while their subjects still breathed, parts that nature had previously hidden, their position, colour, shape, size, arrangement, hardness, softness, smoothness, points of contact, and finally the processes and recesses of each and whether any part is inserted into another or

receives the part of another into itself.")

[221] This is clear from *Med.* 1 pr. 26, *CML* 1.21.29f., where "most people" are said to hold human vivisection to be cruel (see next note), as well as from Celsus' own rejection at pr. 74f., *CML* 1.29.17–22, of vivisection: "to cut open the bodies of living men is both cruel and superfluous; to cut open the bodies of the dead is necessary for medical students. For they ought to know the position and arrangement of parts—which the dead body exhibits better than a wounded living subject. As for the rest, which can only be learnt from the living, experience will itself demonstrate it rather more slowly, but much more mildly, in the course of treating the wounded."

"only a small number of criminals."**[222]** But the Empiricists and Methodists are reported as rejecting human post mortem dissection as well, partly on the grounds that it is, if not cruel, at least nasty (*foedus*),**[223]** but partly also on the basis of the argument that what is observed in the dead is not relevant to the living, since on death the body is changed.**[224]**

The obscure not just in the sense of the theoretical or the speculative, but in the sense of what is literally hidden, cannot or need not be inquired into. So far as anatomy went, Celsus has this to add about the Empiricist position:

> If, however, there be anything to be observed while a man is still breathing, chance often presents it to the view of those treating him. For sometimes a gladiator in the arena or a soldier in battle or a traveller who has been set upon by robbers is so wounded that some or other interior part is exposed in one man or another. Thus, they say, an observant practitioner learns to recognise site, position, arrangement, shape, and such-like, not when slaughtering, but while striving for health.**[225]**

Moreover, to judge from Soranus, the Methodists too showed a certain ambivalence on the question. Dissection is useless, Soranus says in the *Gynaecology* , but it is studied for the sake of "profound learning,"

χρηστομάθεια

.**[226]** So he says he will teach what has been discovered by

[222] *Med.* 1 pr. 26, *CML* 1.21.29–32: *Neque esse crudele, sicut plerique proponunt, hominum nocentium et horum quoque paucorum suppliciis remedia populis innocentibus saeculorum omnium quaeri* . ("Nor is it cruel, as most people state, to seek remedies for multitudes of innocent men of all

future ages by means of the sacrifice of only a small number of criminals.")

[223] *Med.* 1 pr. 44, *CML* 1.24.21f.

[224] On the ancient disputes over dissection, see further Manuli and Vegetti 1977, Vegetti 1979.

[225] *Med.* 1 pr. 43, *CML* 1.24.14–19: *Si quid tamen sit, quod adhuc spirante homine conspectu subiciatur, id saepe casum offerre curantibus. Interdum enim gladiatorem in harena uel militem in acie uel uiatorem a latronibus exceptum sic uulnerari, ut eius interior aliqua pars aperiatur, et in alio alia; ita sedem, positum, ordinem, figuram, similiaque alia cognoscere prudentem medicum, non caedem sed sanitatem molientem* .

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it. "For we shall easily be believed when we say that dissection is useless, if we are first found to be acquainted with it, and we shall not arouse suspicion that we reject through ignorance something which is accepted as useful."**[227]**

Both Empiricists and Methodists thus went some way towards accommodating the findings of dissection. But both probably stopped well short of advocating the continued practice of the method. Here the rejection of dogmatism and speculation was also a rejection of new research. It was left to such a writer as Galen (who, even if he would himself have resisted the label, would certainly have been classed as a Dogmatist by his opponents)**[228]** to recommend the method. This he does in texts whose very eloquence and passion testify not just to Galen's personal commitment to the method but also to his sense of the need to come to its support against its detractors. In *On Anatomical Procedures* he sets out no fewer than four kinds of reasons for studying anatomy:

> Anatomical study has one use for the natural scientist who loves knowledge for its own sake, another for him who values it not for its own sake but, rather, to demonstrate that nature does nothing without an aim, a third for one who provides himself from anatomy with data for inves-

[228] Cf. M. Frede 1981. It may, however, be noted that while Galen does not often admit making mistakes, he does sometimes do so. Thus in *AA* 14.7.214 Duckworth, he does so with regard to operations attempting to reveal the courses of certain nerves. There and elsewhere, when he acknowledges that he was at first unsuccessful in a surgical or anatomical operation, it is often to emphasise the need for practice and experience: cf. *AA* 7.10 (K) 2.621.12ff. (cf. also 3.2 [K] 2.348.14ff., 8.4 [K] 2.674.6ff.). On

occasion, too, he admits to some hesitation on points of detail, for instance concerning the nerves of the brachial plexus at *AA* 15.6.254 (D.).

> tigating a function, physical or mental, and yet another for the practitioner who has to remove splinters and missiles efficiently, to excise parts properly, or to treat ulcers, fistulae, and abscesses.**[229]**

Of these four, it is the last, the practical applications of dissection, that Galen chooses to emphasise here particularly, conducting, at this point, a war on two fronts. First he criticises many of those who practised dissection for concentrating on "that part of anatomy that is completely useless for physicians or that which gives them little or only occasional help," instancing the study of the heart and the bloodvessels communicating with it.**[230]**

> The most useful part of the science of anatomy lies in just that exact study neglected by the professed experts. It would have been better to be ignorant of how many valves there are at each orifice of the heart, or how many vessels minister to it, or how or whence they come, or how the paired cranial nerves reach the brain, than [not to know] what muscles extend and flex the upper and lower arm and wrist, or thigh, leg and foot, or what muscles turn each of these laterally, . . . or where a great or a small vein or artery underlies them.**[231]**

Even the Empiricists, he proceeds, who "wrote whole books against anatomy," have to admit that such knowledge is necessary for physicians, but against the Empiricists in turn Galen pours out his critical scorn.**[232]** Castigating the Empiricist claim that the doctor can learn all the anatomy he needs from the inspection of external lesions he writes: "One might well wonder at their temerity, for since even those who have devoted much time to anatomy have failed to bring the study to perfection, one could scarcely learn it from the contemplation of wounds. Perched high on a throne, a man can say these things to his pupils without being able to instruct them in the actual practice of the art."**[233]** What is needed, he insists, is constant practice on many bodies, aided by the instruction he himself provides in his book.**[234]** The chief motivation for this study and the book itself is clear: "What could be more useful to the physician for the treatment of war-wounds, the extraction of weapons, the excision of bones . . . than to know accurately all the parts of the arms and legs. . . . If a man is ignorant of the position of a vital nerve, muscle, or important artery or vein, he is more likely to be responsible for the death, than for the saving, of his

patients."**[235]**

Conclusions

The controversy over dissection serves to epitomise one dilemma that ancient science faced. Unrestrained or arbitrary speculation, such

[232] *AA* 2.3 (K) 2.288.15ff.

[234] *AA* 2.3 (K) 2.289.17ff.

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as ran riot not just in medicine but in many other areas of the investigation of nature, led in time to a reaction, the rejection of theorising of any kind that went beyond the "appearances."**[236]** Where some Hippocratic writers had already rejected excessive claims for exactness and the use of arbitrary postulates, the Hellenistic medical schools evidently developed clearer and more powerful epistemologies that drew on the traditions of sceptical philosophy. Yet though the sceptic was an inquirer,**[237]** his insistence on the need to withhold judgement and on the idea that it is either impossible or useless to seek to comprehend the hidden causes of nature could and did inhibit, even stop dead, a certain kind of research. The sceptic raised questions and saw that much—in fact he thought *just* as much**[238]** —could be said on either side of disputed issues, but idle curiosity was pointless, and much that had been investigated, in an admittedly often over-sanguine way, had to be rejected as idle curiosity.

On the side of dogmatism, where the dogmatic Hellenistic philosophical sects met the sceptical challenge by upholding one or another positive view of the criterion of knowledge,**[239]** most of the dogmatic

[236] The "appearances" often included the common opinions and beliefs, as well as what was perceived, as already in Aristotle. See Owen 1961/1986, cf. Burnyeat 1977, 1979, 1982b, Nussbaum 1986, pp. 240ff.

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practising scientists took for granted an affirmative answer to the question of whether knowledge is possible. But inordinately speculative theories and excessive claims for their correctness, even their necessity, can be

illustrated in every branch of the inquiry into nature. Many of those who engaged in that inquiry, as we said, pursued the goal of certainty in part under the influence of the models provided by axiomatised mathematics. In the process, much of the complexity of their subject

matter was sometimes ignored, finessed, elided; we shall return to that topic in Chapter 6.

At the same time the example of dissection, especially, shows how it was those who could be criticised for Dogmatism who upheld empirical research. Where the sceptical tradition could degenerate into defeatism**[240]** (even if a defeatism that is readily understandable in terms of the impasse reached in many areas of physical and biological study), it was the more dogmatic and speculative theorists who offered more justification and incentive for further inquiry. It should, however, be stressed that they did so against the background of that challenge from scepticism. The dogmatism in question was, in this respect, still very different from the monolithic traditions exemplified from the ancient Near East in Chapter 2.

Some of the Greek work was, to be sure, undertaken within a framework of regulative principles approximating to what we might call a research programme, and so may be deemed to lend support to the claims of Kuhn and others concerning the role of such in normal science. At the same time we should acknowledge that much ancient speculation had always been and continued to be both more individualistic and more opportunistic than the title *research programme* would suggest or allow. In an ancient perspective, we have seen that whatever inhibiting effects tentativeness and anti-dogmatism came to have, they were also, especially initially, characterised by a notable

[240] Pessimism about reaching satisfactory solutions to the major problems in dispute in physical theory goes back to the pre-Socratic period (see above an nn. 21 and 23) and is thereafter a recurrent theme. One may, however, distinguish between doubts or reservations expressed on particular topics within or after a physical investigation, and a quite general dismissal of the possibility of the study of nature (as is reported for the Cyrenaics, for example, by Diogenes Laertius 2.92, cf. 7.160 on Ariston; and cf. Eusebius *Praeparatio evangelica* 15.62 paras. 7ff., 854c4ff., [2.494 Gifford, 2.423.26ff. Mras], on which see Ioppolo 1980, pp. 78ff.). In late antiquity the failure of science, particularly of astronomy, to secure agreed and consistent results was used by Proclus, for example, to support what he represents as the Platonic thesis, that the only proper objects that can be said to be known are transcendent Forms: see, e.g., *Hyp.* pr. 1ff., 2.1ff., 4.5ff., together with 7.238.9ff., and cf. *In Ti.* 3.56.28ff.; cf. Sambursky 1965.

boldness and originality. In Hippocratic medicine, expressions of uncertainty, statements of the difficulties encountered and of the failures that could have been avoided, at least sometimes reflect a remarkably open and direct response to day-to-day clinical experience and a new commitment to the principle of recording mistakes so that others may learn from them—even if some of these attitudes were themselves in turn conventionalised and became part of the fund of rhetorical commonplaces used by authors who were otherwise unrestrained in their pretensions to knowledge.

Chapter Four— Metaphor and the Language of Science

Metaphor, like mythology, had to be invented—that is to say, the explicit category had to be—and we can trace the steps in which it was made explicit in the fourth century B.C. in Greece. Moreover, our Greek evidence makes it clear that even if there was not quite the scandal that Detienne has recently suggested surrounded the development of the category of myth as fiction,**[1]** the invention of the category of the metaphorical took place against a background of overt polemic. Yet one outcome of the intense debates concerning theories of metaphor in the past few decades has been that increasingly sophisticated challenges have been mounted calling the literal/metaphorical dichotomy itself into fundamental question,**[2]** and this issue has repeatedly been at the centre of the most radical controversies in the philosophy of language, the philosophy of science, and literary critical theory. That in some sense all language is metaphorical has been argued with some force both by literary critics and by philosophers.**[3]** Where theories of mean-

[1] Detienne 1981/1986. For an analysis of Plato's use that diverges in certain respects from Detienne's, see Brisson 1982, and cf. Moors 1982 with Ferrari 1983.

[2] See, for example, Derrida 1972/1981 and 1972/1982. In addition to such classic studies as Black 1962 and Ricoeur 1975/1977, there have been three recent collections of articles in Ortony 1979, Sacks 1979, and Johnson 1981. Shibles 1971 presents an annotated bibliography of work on metaphor to that date.

[3] For one sophisticated statement of such a thesis, see Hesse 1982. Some of the antecedents of such a claim go back, in the English-speaking tradition,to I. A. Richards, who already inveighed against what he dubbed the One and Only One True Meaning Superstition (1936, p. 39), and who saw metaphor as "the omnipresent principle of language" (pp. 92ff.). But radical attacks on the question of metaphor are equally a feature of Continental scholarship, some of which takes its inspiration from Nietzsche, invoked explicitly by Derrida, for instance at 1972/1982, pp. 216f.

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ing in the tradition that stems, precisely, from the Greeks represent the literal and the univocal as the norm, the metaphorical as the deviant, a case as strong or stronger can be made for the reverse reduction.**[4]** The univocal, at least, it can be argued, is the exception; certainly it is not overwhelmingly usual in most natural languages. What proportion of entries in Webster's or Collins' are single entries?

But the more we take note of this recent challenge, the more puzzling the original introduction and invention of that dichotomy are bound to appear. Those who were primarily responsible, Aristotle especially, were in part motivated by the aim of excluding the metaphorical from certain types of discourse. We shall be trying to come to

[4] Standardly, the univocity/equivocity contrast, like that between synonymy and homonymy, is used in the characterisation of terms, the literal/metaphorical contrast of their use, and it can be agreed readily enough that equivocity and homonymy are distinct from, and do not necessarily entail, ambiguity or vagueness in use. But whether it is correct to hold (as Searle has recently argued: 1979, chaps. 4 and 5) that metaphorical meaning is not a property of sentences, but always one of speaker's utterances, is controversial and leads to the heart of the question of the status and validity of the concept of "literal meaning" itself. Thus objections have been brought against Searle by, for example, Hesse (1982, p. 42 nn. 1 and 5), who develops a theory of metaphor based on Wittgenstein's family-resemblance theory of universals, a theory of meaning-as-use that does not recognise as fundamental the distinction between "sentence-meaning" and "utterance-meaning" (for the threefold distinction between utterance meaning, sentence meaning, and word meaning, see, for example, Grice 1968). Hesse accordingly rejects the idea of the literal meaning of a sentence as entirely determined by the meanings of its words and its syntactic rules: "If it is a *matter of fact* that the "literal meaning" of a sentence changes *frequently* in utterance because of metaphoric shifts in the meaning of words in different contexts, then the category of literal meaning becomes applicable only in the same kinds of local or limiting cases in which the category of natural kind is applicable" (Hesse 1982, p. 42 n. 5; original emphasis). For a variety of views on the different types of indeterminacy in terms, sentences, and speech acts, see,

for example, Black 1937; Hempel 1939; Waismann 1945/1951, 1953; Lyons 1977, vol. 1, pp. 169f., 261ff., vol. 2, pp. 396ff., 550ff.; Dammann 1977–78; and see further below, n. 7.

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terms with him later, and in particular to assess the effects of the availability of an explicit category of the metaphorical and to evaluate his demands for the literal and the univocal. Yet first the background to that tradition calls for inquiry, and we shall see that while it is agreed on all sides that Aristotle's influence both in antiquity and subsequently has been immense, his own position is far from being as transparent as those who have used him either as hero or as whipping boy have generally assumed.

Obviously, the use of what will later pass as metaphors antedates the development of the terminology to christen them as such, just as prose did M. Jourdain's discovery that he was speaking it. Equally obviously, the problem of the analysis of the expressions concerned is a delicate one, where questions relating to their status are all too likely to be begged by the application of the battery of dichotomies—literal/metaphorical, primary/derived, strict/figurative and the like,**[5]** —that that terminology tends to generate. It would be better, then (to use a metaphor, but in order particularly to suspend the literal/metaphorical dichotomy),**[6]** to talk of terms with a more or less obvious, more or less deliberate, semantic stretch. This seems preferable partly because it represents the differences as differences of degree (whereas literal and metaphorical are often construed as mutually exclusive and exhaustive alternatives) and partly because it allows the possibility that in use every term has *some* stretch—even, at the limit, any term deemed to be univocal.**[7]**

[5] This is, of course, not to imply that these dichotomies are equivalent. It is a feature of each of them, however, that the first of each pair is generally treated as a norm, the second as a deviation from it. Moreover, these pairs are often seen as each offering a choice between mutually exclusive and exhaustive alternatives.

[7] It has often been said of metaphor that it calls forth or creates a similarity as much as it presupposes one (e.g., Black 1962). But the point can beextended and generalised to allow that within any complex statement there is always an interaction between any one term and the others in the collocation in which it is used. Already at the level of sentence meaning—and leaving aside any considerations to do with speaker's utterance meaning—the syntagmatic lexical relations of each term modify, though of course they do not fully determine, interpretation of the collocation as a whole. What I call "semantic stretch" has often been noted under the rubric of "polysemy" and in connection with theories of "semantic field" (e.g., Porzig 1934), though some commentators have used "polysemy" to mark what they

consider to be a particular feature of certain lexemes, not a pervasive characteristic manifested to a greater or lesser degree throughout all natural languages (see, however, Ziff 1972, p. 70; Ricoeur 1975/1977, pp. 169f.; Hesse 1982).

Early Greek Poetry

Early Greek poetry provides plenty of examples where the stretch of a term seems particularly prominent, a feature that raises the question of how far and at what point the users themselves recognised this; it does not matter, at this stage, that the evidence comes from poetry,**[8]** nor that there may be doubts about the exact connotations of particular Greek terms—if, again, we wish to assume that they *have exact* connotations. Talk about the unseen, the imaginary, the abstract (so often the locus, or the battlefield, of the revolutions of wisdom) is an area where there is bound to be especially heavy demands on semantic stretch. Neither we nor the Greeks can avoid *conceiving* or *grasping* the imaginary with the aid of terms whose stretch reaches back to the perceptible.**[9]** Some of the topics we mentioned in Chapter 1 will serve to illustrate this. The Greeks commonly recognised that some

[8] However, the claimed differences between poetic and would-be scientific discourse will prove a crucial issue later: see below, p. 209f., where note is taken of the fifth-century poets' own problematising of meaning and naming.

[9] The original concrete connotations of much of the vocabulary in which the Greeks expressed abstract ideas were one of the main themes in Onians' 1951 study, where he discussed, in particular, the description of fate in terms of processes of spinning, weaving, and binding (pp. 303ff.) and that of death as a bond tieing a man or as a band or wrapping enclosing or covering him (pp. 327ff., 422ff.). In an earlier study (G. E. R. Lloyd 1966, pp. 192ff.), I already pointed out the difficulty of opting for either a metaphorical or a literal interpretation of such descriptions, though otherwise, no doubt, that discussion was still insufficiently emancipated from the constraints imposed by that traditional dichotomy.

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dreams are true, some false. But was there—where was there?—a Gate of Horn, a Gate of Ivory? The Fates spin and weave your destiny, but what sort of textile was that? Diseases roam silently among men, for Zeus has taken

away their voices. Death binds or covers, sleep is poured over you.

The traditional question that such expressions provoked was: Is this a literal belief, or a metaphor? Yet it should be clear that it is simplistic to force that issue, even though such issues *have* repeatedly been forced, in ancient and in modern times, particularly when insufficient attention has been paid to differences between users' and observers' categories and to the question of the difference it may make to *have* some such *explicit* category as that of the metaphorical.**[10]** Yet before the literal/metaphorical dichotomy is available, while a speaker may have a greater or a lesser sense of some difference between "pour" said of sleep and "pour" said of wine or water, it is truistic to say that the phrase will not be seen *as* a metaphor. It is that dichotomy that erects that particular would-be perspicuous and definite barrier, even though in practice those who wish to erect it generally find it hard to say precisely where it comes—as is shown by the interminability of discussions about the comparative deadness of dead or dying metaphors.

Pre-Socratic Terminology

Problems begin, however, to emerge, even before the terminology of such expressions as

κατὰ μεταφοράν

was forged to press a certain kind of question. We do not, to be sure, find Greek writers suddenly protesting that sleep is *not* poured over men or that death does *not* bind them. But the language available to describe the divine is implicated in Xenophanes' attack on anthropomorphism, even though that attack is still directed at the content of religious ideas rather than at their mode of expression. Nor is it surprising that a challenge that implicitly raises the question of the limits and status of beliefs should

[10] Some aspects of the points I adumbrate here are further developed in my 1985 Rivers lecture at Cambridge (to be incorporated in a forthcoming study).

come in this area, since discourse about the divine is bound to involve exceptional semantic stretch. That discourse cannot afford to do without the use of terms with straightforward applications in mundane contexts, yet just what was being asserted when they were applied to the gods immediately becomes a problem with Xenophanes' denial of the validity of their being so applied. Apart from the moral objections he brings against representations of the gods thieving, committing adultery, and the like,**[11]** he attacks the

whole idea of gods in the form of men. "Men think that gods are born and that they have clothes and voices and shapes like their own."**[12]** "If oxen and horses and lions had hands and could draw with their hands and produce works of art like men, horses would draw the forms of the gods like horses, and oxen like oxen, and they would make their bodies such as each of them had themselves."**[13]**

Familiar as this polemic is, the point that concerns us here is the *literalist* interpretation of representations of the gods that it presupposes. While the belief that the gods form a society like that of men can be paralleled extensively (we need look no further afield than ancient Mesopotamia),**[14]** the extraordinary detail with which the idea is worked out in Homer is exceptional. It is not just that the motivations of the gods—honour, glory, fame—are those of human beings: they engage in human occupations, including weaving and making armour; they scrupulously observe the customs of Homeric society, in the protocol of visits, for example, where a visiting god or goddess is offered a chair and footstool and given something to eat and drink, before being

[11] Xenophanes frr. 11 and 12; cf. above, Chap. 2 at n. 40.

[14] See, for example, Bottéro 1981a and 1981b, and cf. Jacobsen 1949, 1976.

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expected to explain the purpose of the visit.**[15]** They sleep, weep, and sweat, and when they fight they may be wounded and cry out, for all the world as if in physical pain.**[16]** But this very detail, this passage to the limit of the conception of the gods as like humans, opened the way to a reductionist reading (not that such a reading is *faithful* to Homer): the more meticulous the parallelisms, the more "realist" the account might be taken to be, and so the more vulnerable to the criticisms of an admittedly starkly literal-minded Xenophanes.

By itself, the belief that Poseidon is responsible for earthquakes leaves very vague the answers to the questions of how he does it and why he did it on a particular occasion (though the form of the answer in the second case would naturally be given by the assumption of quasi-human intentionality, his more or less inscrutable will). But the more vividly Poseidon is imagined as sitting down to table on Olympus (even if to a meal of nectar and ambrosia) the more difficult the *how* question will be to answer or, rather, the harder it will be to ignore. For Atlas, similarly, to hold the earth up, he had better not be *too* anthropomorphic. Nor Zeus when he rains or thunders. At one stage, no doubt, all Greeks simply knew that Zeus rains. But when combined with anthropomorphism pushed to the limit, such an item of traditional religious knowledge could be challenged not just on the grounds of consistency, but

also with a demand for clarification concerning precisely what was being asserted.

Xenophanes himself, we should recall, continues to describe god's behaviour in terms that are also used of men: god sees as a whole, thinks as a whole, hears as a whole, and he sways all things by the thought of his mind, without effort.**[17]** But the important difference is that Xenophanes guards himself against a too literal interpretation of *his* religious propositions (even though *we* may still have questions

[15] See, for example, the descriptions of the way in which Thetis is greeted by the other gods at *Il.* 24.97ff., or Hermes by Calypso at *Od.* 5.85ff., or Charis by Hephaestus at *Il.* 18.382ff., and cf. Menelaus' reception of Telemachus at *Od.* 4.30ff., or Alcinous receiving Odysseus at *Od.* 7.167ff.

[16] See, for example, *Il.* 5.855ff.; cf. 4.27.

[17] Xenophanes frr. 24 and 25.

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enough to put concerning both the meaning and the consistency of his statements about the gods). In the operations of the divine mind certain rules that apply to merely human thought are deliberately suspended; for we cannot change the world, or sway all things, just by taking thought. Fragment 23 is explicit on the point, for there the single god ("greatest among gods and men") is said to be unlike men not just in form *but* also in thought.**[18]**

After Xenophanes, what had been true of much early religion *remains* true of theology, of cosmology, and of other areas of philosophical thought where there is obvious pressure on the semantic stretch of terms, and already in the next generation of philosophers there are increasing signs of a conscious recognition of departures from common usage along with the beginnings of a far-reaching problematising of the relationship between language and reality as a whole. The general point is well known and need not detain us long. The paradoxes of Heraclitus provide many fine examples: war, we are told, is father of all and king of all;**[19]** all human laws are nourished by the single divine law;**[20]** nature loves to hide;**[21]** thunderbolt steers all.**[22]** From the Eros of Parmenides' Way of Seeming, through the Love and Strife of Empedocles and Mind in Anaxagoras, to Mind, again, in Diogenes of Apollonia,**[23]** the history of pre-Socratic cosmological speculation is a history of what we find it tempting to assume we can straightforwardly call images, metaphors, or analogies,**[24]** although, strictly speaking, it would be better not to use terms that might suggest that their authors viewed them *as such* or, indeed, that *they* had some clear alternative.

[19] Heraclitus fr. 53, cf. fr. 80.

[20] Heraclitus fr. 114.

[21] Heraclitus fr. 123.

[22] Heraclitus fr. 64.

[23] Parmenides fr. 13, Empedocles frr. 17 and 35 especially, Anaxagoras frr. 12 and 13, Diogenes of Apollonia frr. 3–5.

[24] I would now wish to be rather more cautious in applying such characterisations than I was in G. E. R. Lloyd 1966.

Quite *how* their authors saw them, and how far and how explicitly they recognised problems to do with the meanings of certain terms, are indeed crucial, if delicate and at points not ultimately decidable, questions.

Some direct problematising of language can, however, be illustrated already in Heraclitus, for whom "the one wise thing is not willing, and is willing, to be called by the name of Zeus,"**[25]** and for whom the name of the bow is life (βίος, one name for the bow being βιός), but its work is death.**[26]** In Parmenides the attack on certain terms takes the form of the charge that they are vacuous, with no purchase on reality. "Coming-to-be and perishing, being and not being, change of place and alteration of bright colour" are names laid down by men confident that they are true, but the only thing there is to be named is what is,**[27]** and for Empedocles and Anaxagoras too coming-to-be and perishing are empty terms, merely conventional expressions.**[28]**

Of all the pre-Socratic philosophers, Empedocles, perhaps, comes closest to an explicit recognition of the extension involved in his use of a term for a cosmological principle, for he says of Philia, Love, that while she is acknowledged as inborn in the limbs of mortals and is called by the names of

Joy and Aphrodite, yet no man is aware of her as she goes to work on the elements.**[29]** Yet however imperfect our ideas

[25] Heraclitus fr. 32.

[26] Heraclitus fr. 48.

[28] Empedocles frr. 8 and 9, Anaxagoras fr. 17.

may be, it is that cosmic principle that is at work in us. Cosmic Love would have to be said to be no mere metaphor, then, if we chose to press that question, but that just points to the difficulty of the stretch involved in the application to cosmology of any such term, and that in turn says something about what it is to do cosmology.

Plato

By the time we reach Plato not only is there an extraordinary proliferation of images and analogies (now often recognised as such) deployed in cosmology, psychology, politics, and ethics, but their use, or some of their uses, become the subject of explicit comment.**[30]** Though the terms *muthos* and *logos* are not always contrasted, of course, they can be used, and were (notoriously) by Plato, to indicate a difference in the statuses of accounts.**[31]** A *logos* can be, and in certain cases should be, incontrovertible, a matter of demonstration or at least of verification and argument: a *muthos* may be believed to be true and yet be incapable of proof (though many *muthoi* are presented as mere fictions).

Though *muthoi* have their uses, one refrain from the Socratic dialogues onwards is a demand for definition, for clarity, for the giving of

[30] Among the important studies of various aspects of this topic in Plato, see especially Goldschmidt 1947a; R. Robinson 1953, pp. 202ff.; Bambrough 1956/1967 and 1962/1967; J.-P. Vernant 1979, pp. 105ff.; Detienne 1981/1986; Brisson 1982; Ferrari forthcoming. Some issues are discussed in my 1966, pp. 389ff.

[31] Some examples of this were given above, Chap. 1 at nn. 26–30, and cf. Chap. 3 at nn. 115–16.

account. In texts in the *Phaedo, Phaedrus, Theaetetus* , and *Sophist* , especially, aspects of the use of images,

εἰκόνες

, likenesses,

ὁμοιότητες

, and the plausible and specious,

πιθανολογία

, are discussed critically, with warnings as to the possible deceptiveness of all of these and to their inadequacy as a method of proof.**[32]** Here, then, are certain general statements concerning the validity of certain types of argumentative device. Even so none of these texts offers an explicit *definition* of the arguments in question, let alone a formal analysis of the type Aristotle was to undertake in connection with his theory of the syllogism. Although in the *Sophist* , especially, Plato begins the analysis of otherness, difference, contrariety, similarity, and identity, he undertakes no systematic classification of those relationships, nor does he directly investigate the relationships between the various modes of reasoning that we may say are based on implicit or explicit comparisons. Moreover, when he says that accounts that use images are charlatans,

ἀλαζόνες

,**[33]** we ignore at our peril that he uses a likeness to tell us that likenesses mislead. Or, again, when in the *Sophist* we are told that likenesses are a "most slippery tribe,"

ὀλισθηρότατον γένος

, we might ask how slippery that characterisation is.**[34]**

Plato's ambivalence on this whole topic emerges not just from his

[32] See especially *Phd.* 92c–d, *Phdr.* 262a–c, *Theaetetus (Tht.)* 162e–163a, *Sph.* 231a–b, 236a–b, 240a ff.

own very extensive *use* of similes, metaphors, and analogies, but *within* those explicit comments on likenesses, for while some texts issue warnings about their deceitfulness, others recognise their usefulness. Paradigms, especially, are allotted a positive role,**[35]** both for didactic purposes, to bring a student to an understanding of a difficult problem by considering first a simpler case or one analogous to it,**[36]** and for heuristic ones, where the dialectician himself is supposed to use a similar method to discover the truth.**[37]**

Aristotle's Critique of Metaphor

In Aristotle, the shift in emphasis towards a more negative evaluation—at least in certain contexts—is marked. First, he frequently censures the metaphors and images used by his predecessors. Thus Empedocles' notion of the salt sea as the sweat of the earth is "adequate, perhaps, for poetic purposes" but "inadequate for understand-

[35] See especially Goldschmidt 1947a.

[36] When the use of paradigms is itself illustrated and explained by a paradigm in the *Politicus* , 277d ff., the Eleatic Stranger takes the case of children learning to read. Once they have learnt to recognise letters in short and easy syllables, they can be taught to recognise them also in more complex combinations, by juxtaposing the known and the unknown and pointing to the same likeness and nature in both cases. In both the *Sophist* and the *Politicus* , paradigms serve to provide practice in method, when the method of division, to be used on the sophist and on the kingly art, is first exemplified with the easier cases of angling (*Sph.* 218e ff.; cf. 218b–d) and of weaving (*Plt.* 279a ff.; cf. 286a–b).

[37] In the illustration of children being taught to read, the instructor himself already knows the letters. But in the problems investigated in the *Sophist* and *Politicus* , the hunt for the sophist and for the definition of the kingly art are represented as *searches* , where neither the leader, the Eleatic Stranger, nor his interlocutors have the answers when they set out. Moreover, in both dialogues the paradigm that is chosen as an illustration is particularly relevant to the substantive subject under investigation. In the *Sophist* , when the activity of dividing angling illustrates the method, angling turns out to be like sophistry (*Sph.* 221d8ff.), and in the *Politicus* weaving is chosen at the outset (*Plt.* 279a7ff.) for its similarity to politics (cf. *Plt.* 308d ff.). In these examples the activity in the case of the paradigm is an instance, not merely a likeness, of the activity also exemplified in the larger case.

ing the nature of the thing."**[38]** Other images of Empedocles and other pre-Socratic philosophers are criticised on the grounds that they are based on superficial similarities—or on none, that the illustrations are obscure, or crude, or in need of qualification.**[39]** Thus milk, he insists at one point, is formed by a process of concoction, not putrefaction, so Empedocles was wrong, or he used a bad metaphor, when he spoke of it as "whitish pus."**[40]** Similarly, Plato's own theory of Forms as a whole is dismissed on the grounds that to say that the Forms are "models and that other things share in them is to speak nonsense and to use poetic metaphors"**[41]** — where again we may remark that *poetic* is used as a term of censure.

Aristotle is especially uncompromising in his criticisms of the use of

μεταφορά

in the context of his formal logic and theory of demonstration,

μεταφορά

, for him, being defined as the transfer of a term appropriate to one domain to another.**[42]** In the *Posterior Analytics* he condemns them as a whole, especially their use in definitions. "If one

[39] See, for example, *Top* . 127a17ff., *GA* 747a34ff., 752b25ff., and cf. *De sensu (Sens.)* 437b9ff., *PA* 652b7ff. Cf. Bremer 1980.

[41] *Metaph.* 991a20ff., 1079b24ff. Other comparisons used by Plato are criticised at *Pol.* 1264b4ff., 1265b18ff., for example.

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should not argue in metaphors, it is clear that one should not use metaphors or metaphorical expressions in giving definitions."**[43]** In the *Topics* , too, he repeats the criticism of definitions that contain metaphors on the grounds that "every metaphorical expression is obscure."**[44]**

There is, to be sure, another side to the picture. Elsewhere when he discusses style, especially,**[45]** he approves of certain types of metaphor, particularly those that express a proportion, for these, he says, are vivid, witty, and clear**[46]** (by which he does not mean to deny that from another point of view they are still "obscure"). He praises in the poet the ability to

deploy metaphor and to discern resemblances; the latter is a skill that the philosopher too will need to exhibit.**[47]** In the *Topics* , moreover, the "investigation of likeness" is said to be a useful means by which to become well-supplied with arguments and even also, in certain contexts, for rendering definitions, that is, in securing the genera for them.**[48]** In the *Sophistici Elenchi* he is not above recommending

[45] See *Rh.* 1405a8ff., 1407a14ff., 1410b36–1411b23.

[46] For example at *Rh.* 1405a8ff., 1410b13ff.

[48] *Top.* 105a21ff., 108a7ff., b7ff.

metaphor as a way of making an account difficult to refute without, he hopes, being found out—a backhanded recommendation, to be sure.**[49]**

Nevertheless Aristotle's appeal to the contrast between the "proper" and the "alien" or transferred uses of terms runs counter to some modern preoccupations. It leaves little room for any concession to an interaction view of metaphor or for the idea that a metaphor may create a similarity as much as show one.**[50]** Indeed, the assumption in the background is that the comparisons implicit in proportional *metaphora* can be spelt out fully in literal terms without loss: his notion of *metaphora* already presupposes that there are two distinct, independently identifiable fields between which a transfer has taken place and in only one of which the term transferred is "proper." Finally, his analysis of analogical argument in the form of the paradigm concentrates on its shortcomings judged from the standpoint of the theory of the syllogism.**[51]** It proceeds from particular case to particular case, whereas for the argument to be valid it must proceed first by a complete induction to a universal rule, which is then applied deductively to the particular case in question in the conclusion.**[52]**

The concessions that Aristotle makes, from time to time, to the usefulness of various modes of reasoning based on likenesses do not do much to mitigate an attitude that is otherwise strongly critical. But these formal condemnations provoke a series of questions. First, as regards Aristotle himself, how far does his actual practice tally with the implications of those formal condemnations, and insofar as it does not,

[49] *SE* 176b20ff., 24f. In other texts too Aristotle describes how the dialectician may exploit similarities to deceive an opponent, especially at *Top.* 156b10ff., *SE* 174a37ff., and he reverts with great frequency throughout the *Topics* to the topic of the inspection of similarities in general,

e.g., 114b25ff., 124a15ff., 136b33ff., 138a30ff.

[51] See especially *APr.* 2.24.68b38ff. Paradigms are also discussed, from the point of view of their use in rhetoric, at *Rh.* 2.20.1393a22–1394a18, and cf. 1357b25ff., 1368a29ff.

[52] See *APr.* 69a13ff.; cf. *Rh.* 1357b26ff.

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how can we explain the driving force behind his critique? Is it really the case that he entirely purges his speculative thought of metaphor, heeding his own warning that "every metaphorical expression is obscure"? Does he quite fail to recognise the role of models in both philosophy and science? What kind of impoverished science would it be that did without theoretical terms drawing, implicitly or explicitly, on models and even *metaphorai?* But if and when he in fact uses such terms, how could he square them with his theory of definition and demonstration? The questions may be raised in relation to Aristotle himself in the first place, particularly with regard to his contributions to the inquiry concerning nature. But this in turn will lead us to broach similar issues in relation to the use of theoretical and technical terms, and the role of metaphors and models, in Greek science more generally.

The Art of Nature

We may concentrate first on the inquiry into nature in Aristotle, and the term "nature" itself offers an excellent starting-point. As is well known, nature is defined in terms of an innate capacity for movement, and the power at work in what has that capacity (especially living creatures) is captured in a wealth of images, comparisons, and analogies. When Aristotle describes the growth of the embryo or the structure of the bones or the blood-vessels in the body or the way in which the blood is used as the material for the other parts in the body, he compares nature to a modeller in clay,**[53]** to a painter (sketching a figure in outline and then applying the colours),**[54]** to a housebuilder (laying out the stones along the foundations of the house),**[55]** and to a good housekeeper (not wasting material).**[56]** Several more images are borrowed directly from, or at least echo, those that Plato had used when describing

[53] *PA* 654b29ff.; cf. *GA* 730b27ff. and other comparisons incorporating the idea of the construction of a framework for a model, e.g., *HA* 515a34ff., *GA* 743a1ff.

[54] *GA* 743b20ff.; cf. 764b30f.

[55] *PA* 668a16ff.

[56] *GA* 744b16ff., *PA* 675b20ff.

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the work of the Craftsman in the *Timaeus* . Aristotle too compares the blood-vascular system to a system of irrigation channels,**[57]** and he too compares the crisscrossing of the blood-vessels to a wickerwork structure.**[58]** More simply, nature is repeatedly described as creating,

ποιεῖν,

δημιουργεῖν

, devising,

μηχανᾶσθαι

, and adorning,

ἐπικοσμεῖν

, living creatures or their parts,**[59]** and most frequently of all, of course, her purposeful activity is expressed in the phrase "nature does nothing in vain,"

ἡ φύσις οὐδὲν μάτην ποιεῖ

.**[60]**

For a philosopher who condemned all metaphor as obscure, Aristotle is, one might think, extraordinarily free with implicit and explicit comparisons of every kind between the role of

φύσις

and the

τέχναι

. But the first-stage defence he would offer is not far to seek. It is above all in relation to the workings of the final cause that these comparisons are developed. *Both* domains, Aristotle would insist, exemplify finality, though its modality in each is different: he points out, for instance, that nature does not deliberate, just as he also recognises that there are exceptions to finality, failures to secure the good, in both artistic and natural productions.**[61]** But in many of the comparisons he draws he would claim that there is no question of transferring conclusions from one particular instance *to* another *directly* (thereby encountering the difficulty he mentioned in his analysis of analogical argument). Rather, both particulars fall under a general rule for which he believes he has ample grounds. Art can be used to illustrate nature because both domains manifest certain general principles concerning, for example, the adaptation of form to function, the hierarchisation of ends, and the relationship between the end to be attained and the character of the matter necessary to attain it. To quote just one prominent example:

[57] *PA* 668a13ff.; cf. Plato *Ti.* 77cff.

[59] E.g., *GA* 731a24, *PA* 652a31, 658a32.

[60] Bonitz's index, which is not exhaustive, cites twenty-three instances from the Corpus, 836b28ff. Cf. the discussions of Ulmer 1953; Solmsen 1960, pp. 102ff.; Bartels 1966; Fiedler 1978.

[61] E.g., *Ph.* 199a33ff., b26ff.

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just as an axe, to be used for chopping, must be made of a hard material such as iron or bronze, so each of the parts of the body must be of a material suitable for the function it is to perform.**[62]**

But if, in general, we can see why art may be invoked as an analogue to nature, this does nothing to explain why in any given case a particular technological analogy should be used, let alone guarantee that it will not mislead. The crisscrossing of the blood-vessels may suggest wickerwork, but it does not show that they do indeed have the function of binding the front and the back of the body.**[63]** Moreover, in this instance there is a fairly obvious negative analogy (or difference) that might have given Aristotle pause, in that the texture of the blood-vessels, the veins especially, might be thought ill suited to serve a binding function.

An even more disastrous example is Aristotle's theories concerning the role of the testicles, which he several times compares to the weights on looms.**[64]** He believes their function to be, not to produce the semen but, rather, simply to keep the seminal vessels taut. It is true that he believes he

has independent evidence that even after castration bulls can fertilise cows successfully, a supposed fact that he took to suggest that the testes do not produce seed.**[65]** The tension of the seminal vessels, on the other hand, would—he thought—be released only gradually after the excision of the testes. The loom-weight idea offered the basis of an alternative theory, though the more immediately visible similarity it appealed to was—*we* should say—superficial.

Again, the general doctrine of the adaptation of the parts of living creatures to ends is expressed by Aristotle with the help both of particular comparisons with

ὄργανα

, tools or instruments, and of the term

ὀργανικόν

, instrumental, applied to such non-uniform parts as the hand. When he speaks of the organs of the body, the technological model plays an active heuristic role. A single text will serve to illustrate

[62] *PA* 642a9ff.; cf., e.g., 639b23ff., 646a24ff., b3ff.

[63] *PA* 668b21ff. Aristotle does, however, also hold that the blood-vessels serve to convey nourishment to the parts of the body, e.g., 668a12–21.

[64] E.g., *GA* 717a34ff., 787b19ff., 788a3ff.

[65] *GA* 717b3f.; cf. *HA* 510b3f.

the doctrine: "since every instrument (

ὄργανον

) is for the sake of something, and *each of the parts of the body is for the sake of something* , that is to say, some action, it is clear that the body as a whole arose for the sake of some complex action. *Just as* the saw came to be for the sake of sawing, and not sawing for the sake of the saw . . . *so* the body exists for the sake of the soul in a way and the parts of the body for the sake of the functions that

each of them naturally fulfils."**[66]**

Definition of Terms

In many of the cases so far considered, Aristotle would justify the implicit or explicit comparisons he himself uses by referring to the general rule, of which both items compared can be seen as instances, a rule which can, or should in principle, be supported independently. But the broader questions that Aristotle's theory of meaning and his demand for precision and the literal raise concern also his reaction to and criticism of many of the complex and problematic theoretical terms that his predecessors and contemporaries used in their natural philosophical speculation, whether or not Aristotle saw these as, or as involving, metaphor. In some instances he proceeds in the way we might expect from his criticisms of the obscurity of metaphor and the like and from his general statements requiring the strict use of terms: that is, he goes all out to purge the terms of ambiguity and vagueness and to establish a single clear-cut definition, even though the strain

that this imposes on some parts of his scientific enterprise are, at times, as we shall see, considerable. In other cases, however, he allows that a term may be "said in many ways,"

πολλαχῶς λεγόμενον

, but argues that these ways have a systematic relationship to a single central, "focal" meaning, a principle particularly important, as Owen showed,**[67]** in relation to many high-level metaphysical concepts such as essence (

τὸ τί ἦν εἶναι

), being, and substance themselves. The question that this raises is the extent to which this type of analysis implicitly modifies the ideals set out in the *Organon* . We may consider first two pairs of examples from his physics, heavy/light and hot/cold, to illustrate the former type of move and to analyse its strengths and weaknesses.

The pair heavy/light had been used in ordinary Greek primarily of what is difficult or easy to carry, though in both cases with a fair range of other meanings or applications as well, including difficult, and easy, more generally.**[68]** But signs of the strain under which the naive conception was coming are already visible in pre-Socratic philosophy, where various correlations are proposed with other pairs of opposites (such as dense/rare) or with the elements as well as with movements,**[69]** and

[67] See especially Owen 1957/1986, 1960/1986, 1965a/1986.

[69] See, for example, Parmenides fr. 8.56ff., Empedocles fr. 21, together with the admittedly often tendentious reports and criticisms in Aristotle (e.g., *De generatione et corruptione* [*GC*] 314b20ff., 315a3ff., 10f.), and in Theophrastus (*De sensu, Sens.* , e.g., 59ff.). Whether or not weight is a primary property of the atoms for Leucippus and Democritus is much disputed (see below, Chap. 5 n. 41), but it appears from passages in both Aristotle (*GC* 326a9f.) and Theophrastus (*Sens.* 61) that heavy and light were sometimes referred to the size of atoms. Compounds, however, could also be distinguished by the proportion of atoms to void or the amount of void they contain. Theophrastus also reports (*Sens.* 62, 68) that light and heavy were correlated or associated with rare and dense (as also still in Aristotle *Ph.* 217b11ff.).

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where Aristotle complains with some justice that the capacities in question were generally left undefined.**[70]**

Plato in the *Timaeus* first follows up the popular association with below and above and emphatically rejects the idea that this second pair relates to two distinct regions in the universe.**[71]** The universe is spherical, so it makes no sense to talk of one part of the sphere being above or below another. Imagining—boldly—a thought experiment in which someone stands in the heavens at the interface of fire and air and forces a larger, and a smaller, quantity of fire towards the air (i.e., towards the centre), he says that it is obvious that the smaller quantity will be moved more easily.**[72]** It then will be "lighter" and tend "upwards," the larger will be "heavier" and tend "downwards"—though "downwards" in this case is *to* the periphery, "upwards" away from it, whereas the ordinary Greek assumption was that, on earth at least, more fire is "lighter."**[73]** What is light in one region, Plato is prepared to say,**[74]** is the opposite of what is light in the other. Several aspects of the interpretation of this text remain highly disputed,**[75]** but it is beyond doubt that Plato has radically redefined heavy and light: they do not just depend on the quantities of the material concerned but, like up and down, are relativised to where in the universe you are or to which element is in question.

Aristotle, in turn, is no less emphatic that certain conventional views are mistaken. Modifying Plato's idea of the importance of the element in which the real or imagined weighing takes place, he distinguishes between the two simple bodies that are heavy (or light) absolutely (that is, earth and fire) and the two that are so only relatively

[70] *De caelo (Cael.*) 308a3f.; cf. Theophrastus *Sens.* 59f., who makes partial exceptions of Democritus and Plato.

[71] *Ti.* 62c ff.

[72] *Ti.* 63b–c.

[73] This is stated to be obviously true and to hold universally, by Aristotle at *Cael.* 308b3ff., 13ff., 18ff., in the course of his quite exceptionally polemical criticisms of the theories in the *Timaeus* .

[74] *Ti.* 63d–e.

[75] See, for example, Solmsen 1960, pp. 275ff.; Hahm 1976, pp. 59ff., 70; O'Brien 1984.

(water and air: that is, relative to other elements).**[76]** He is confident that air is light in comparison with earth and with water, but he raises as a puzzle the question of whether air has weight in air, deciding the issue positively by invoking a purported trial which, he claimed, showed that in air an inflated bladder weighs more than an uninflated one.**[77]** Evidently here the possibility of carrying out a measurement was appreciated, though its difficulty and delicacy are reflected in the fact that when Aristotle's conclusion was challenged by later commentators, first by Ptolemy and then by Simplicius, they obtained quite different results from the same test.**[78]**

Aristotle further diverges from Plato in insisting that heavy (and down) are always to be defined in relation to movement *to* —and light (and up) in relation to movement *away from* —the centre of the universe, deemed to coincide with the centre of the earth.**[79]** Plato too had held that the universe and the earth are spherical, but Aristotle now demonstrates the latter thesis with a battery of arguments.**[80]** Some of these, it is true, are not independent of the issue concerning the nature of heavy and light, for they attempt to show the earth's sphericity as a *consequence* of the doctrine that the natural movements of the simple bodies are to, or from, the centre of the universe, where Aristotle *assumes* that heavy bodies do not move downwards in parallel lines.**[81]**

[76] See, for example, *Cael.* 4.1.308a7ff., 4.4.311a15ff., and cf. Seeck 1964, pp. 108ff.; Hahm 1976, p. 62.

[77] *Cael.* 311b9ff.

[78] Simplicius reports Ptolemy's result (that the bladder weighs less) and then proceeds to describe his own attempt to verify the facts at *In Aristotelis*

De caelo commentaria (*In Cael.*) 710.24ff. The weight of a bladder inflated with air is also discussed in the *Problemata* , 25.13.939a33ff., and in Anon. Lond. 31.34ff., 32.22ff.

[79] E.g. *Cael.* 308a14ff.

[80] *Cael.* 297a8–298a20; cf. *Ti.* 62e, 63a. The importance of this point will be the greater if Furley 1976, pp. 97f., is right to argue that the rival atomist account of the natural motion of atoms (in Epicurus, certainly, and in Furley's view also in Democritus), according to which they move perpendicularly "downwards" in space, depends crucially on the doctrine that the earth is flat.

[81] *Cael.* 296b6ff., 18ff., 297b17ff.

But some offer good independent grounds for his thesis, notably arguments that appeal to astronomical data, first, to changes in the visibility of the stars at different latitudes, and especially in the circumpolar stars that never set,**[82]** and, second, to the shape of the earth's shadow in eclipses of the moon.**[83]**

The example of heavy and light vividly illustrates the meaning shifts that occur as theory develops, shifts that are similar in kind to those that have been explored from later science, where one example often cited is that between the notions of mass in Newton and in Einstein.**[84]** While in assessing just how radical those ancient meaning shifts were it is fair to recognise that the theoretical framework within which heavy and light were entrenched in ancient debate was a good deal less sophisticated than many more modern examples, we should not, on the other side, underestimate just how much of Aristotle's account of both the sublunary and the superlunary region was at stake—a point not lost on some of his ancient critics such as Philoponus.**[85]** Meanwhile Aristotle's own view of the matter was that he was providing heavy and light with clear, univocal definitions, and ones that incorporated the adjustments to popular notions necessary to take into account the doctrine of the spherical earth.

My second example was the pair hot/cold. Once again Aristotle

[82] *Cael.* 297b30ff.; cf. further below, Chap. 5 at n. 57.

[83] *Cael.* 297b24ff.

[84] See, for example, Kuhn 1964/1977, p. 259 n. 30. Aspects of the problems of meaning invariance have been discussed, taking as illustrations the differences between Aristotle and his predecessors on the question of up and down, falling and rising, by Feyerabend 1962, p. 85 (1981a, pp. 85ff.), and by Hesse 1974, pp. 33ff.

[85] In his *De aeternitate mundi contra Aristotelem* , for which our chief source is the extensive quotations in Simplicius, Philoponus explores, among other things, the difficulties that Aristotle's theory encounters in squaring the doctrine of the four simple bodies with that of the two directions of natural sublunary movement, and he mounts a sustained attack on the Aristotelian doctrine of the fifth element, aether, lacking the primary qualities hot, cold, wet, and dry. See especially Wildberg forthcoming and cf. also the *De Aeternitate mundi contra Proclum* 13.6 and 13–17, 492.5ff., 512.17–531.21; cf. M. Wolff 1978, p. 156.

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complains about the ambiguities of common usage—and about the disagreements among earlier theorists.**[86]** Sometimes touch is invoked as the criterion, sometimes various effects (melting, burning, and the like) that the substance claimed to be hot, or cold, has on other things, and the conflicts between these criteria are discussed. Thus, boiling water imparts heat better than flame, but flame can burn; again, boiling water, he says, is hotter to the touch than olive oil, but cools and solidifies more quickly.**[87]** The consequences of unclarity on this, and on the nature of the dry and the wet, are particularly drastic since, as he puts it, "it seems evident that [these four primary opposites] are practically the causes of death and of life, as also of sleep and waking, of maturity and old age, and of disease and health."**[88]** More even than that, they provide the basis of Aristotle's own essentially qualitative element theory.

In the *De generatione et corruptione* he presents not only a very full discussion of issues connected with element theory and of rival views to his own, but also a set of definitions of the four primary opposites, to which he believes other qualitative differences (hard and soft, rough and smooth, viscous and brittle, and so on) can be reduced.**[89]** "Hot," he says, is "that which combines things of the same kind" (

τὸ συγκρῖνοντὰ ὁμογενῆ

), "cold," "that which brings together and combines homogeneous and heterogeneous things alike"(

τὸ συνάγον καὶ

συγκρῖνον ὁμοίως τά τε συγγενῆ καὶ τὰ μὴ ὁμόφυλα

). Again, "wet,"

ὑγρόν

(though "fluid" is often a better translation) is "that which, being readily delimited [i.e., by something else], is not determined by its own boundary," and "dry" (or solid) is "that which, not being readily delimited [i.e., by something else], is determined by its own boundary."**[90]** Aris-

[86] See, for example, *PA* 648a21ff., 36ff., and cf. also 649b9ff., *GC* 330a12ff., on dry and wet.

[87] *PA* 648b12ff., 17ff., 26ff., 30ff. Aristotle further exploits the distinction between what is hot *per se* and what hot *per accidens* , e.g., *PA* 649a5ff., and between what is hot potentially and what hot actually, e.g., *PA* 649b3ff.

[88] *PA* 648b4ff.

[89] *GC* 2.2.329b7ff.

[90] *GC* 329b26–32.

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totle does not proceed *per genus et differentiam* , but he evidently aims to give clear and distinct characterisations of the four primary opposites. The somewhat abstract nature of his account is, however, striking. Moreover, as soon as we look at the range of types of case where he uses the four opposites, we encounter instances where his initial characterisations seem inappropriate and hard to apply.

This is particularly true when he is discussing the role of vital heat, one of the chief foundations of his whole biology. It is important, from his point of view, that it is *heat* in question, since this gives him his link with his general physical theory of the elements. But it is a quality that sometimes seems remote both from anything that might be suggested by the definition "that which combines things of the same kind" and from what might be thought to have some justification either in terms of popular usage or, indeed, of appeals to subjective impressions. Thus in one of his several discussions of the main groups of animals**[91]** he arranges them in a hierarchy according to their methods of reproduction, which are themselves correlated with the

four primary opposites. The most perfect animals, the Vivipara, are "hotter and wetter and less earthy by nature"; next come the ovoviviparous animals, the cartilaginous fishes (sharks and rays), which are cold and wet; the third and fourth groups are Ovipara that lay perfect, and those that lay imperfect, eggs, and these are hot and dry, and cold and dry, respectively; and the fifth and final group, the larvae-producing animals such as the insects, are "coldest of all."

We can see why he claims that the Vivipara, which include humans, are the most perfect creatures, and also why they are warmer than, for instance, fish. Yet it is a puzzle why he should claim that the oviparous

fish are cold and *dry* —though we may notice that he has already used the combination cold and wet for the (superior) ovoviviparous fish.**[92]** The whole represents a schema that appears to owe more to Aristotle's preconceptions of the hierarchy of the animal kingdom, and especially to his views on the distance of the different groups from humans at the top of that hierarchy, than it does *either* to empirical considerations, or even to considerations derived from the general definitions of hot, cold, wet, and dry set out in the *De generatione et corruptione* .

Nor is it only in connection with animal taxonomy and vital heat that questions of this kind arise. At the very heart of the physical theory, some of the correlations proposed between simple bodies and pairs of primary opposites pose problems. Earth, Aristotle suggests, is cold and dry, air hot and wet, water cold and wet, fire hot and dry.**[93]** The "wetness" of both air and water corresponds, of course, both to the range of the term

ὑγρόν

in normal Greek usage and to Aristotle's definition as "that which, being readily delimited [by something else], is not determined by its own boundary." Yet conversely, while, for the sake of the schema, fire has to be hot and *dry* , and "dry" may seem unproblematic enough at first glance, when we reflect on his definition of that quality it becomes much harder to see its appropriateness as a characterisation of *fire:* for just as

ὑγρόν

corresponds rather to fluidity than to wetness, so

ξηρόν

as "that which, not being readily delimited [by something else], is determined by its own boundary," often corresponds to *solidity* , and so from that point of view does not look very suitable for fire.**[94]**

[92] Compare, however, the account given of the dietetic qualities of different sea-animals at *Vict.* 2.48 (L) 6.548.9ff., where fish in general, and also shellfish, are said to be "dry," though the cartilaginous fish "moisten" ([L] 6.550.7f.). But in the discussion in the medical writer many other qualities are also taken into account, and differences are suggested between the flesh of different types of fish.

[93] *GC* 330b3ff.

[94] Cf. *GA* 761b18ff. and the discussion of the shape of flame in Theophrastus *Ign.* 52ff., 35.6ff. While Theophrastus represents flame as generally having a pyramidal shape, he recognises also not just that fire is a kind of movement (see above, Chap. 3 at n. 170), but also that flame is constantly moving and flowing (*Ign.* 54, 37.3ff.).

Focal Meaning, Proportional Analogy, and Homonymy in Aristotle's Science

Heavy and hot are, then, two terms of great theoretical importance where, diagnosing confusions in their use, Aristotle aims to establish and adhere consistently to a single univocal definition but in practice encounters difficulties in following through this programme. Elsewhere, however, as we said, he uses the concept of focal meaning, which preserves the centrality of a primary significance but allows a cluster of others to be related to it. We cannot do justice here, clearly, to the intricacies of this important concept, and of the related but distinct notion of proportional analogy, but one of Aristotle's canonical examples will serve as the briefest of introductions.**[95]** "Healthy," we are told,**[96]** is said primarily in relation to health itself, but also derivatively of signs of health (as when a blooming complexion is said to be healthy) or of what promotes or preserves health (as when regular exercise and a kind of climate are said to be healthy). "Healthy" is not to be understood and explicated in the same way when said of a climate or of exercise as when said of a patient who has recovered from illness, but the term is not merely ambiguous or homonymous, since all the other uses are to be connected with a primary one in relation to however we define health itself. This allows for what I have been calling semantic stretch, while it still privileges a primary application.

Here, then, is a device of great power and scope which Aristotle in fact uses

repeatedly and to particular effect, as we noted, in connection with some high-level metaphysical principles such as essence and being. In his *Physics* and elsewhere such concepts as place,

τόπος

, or what it is to be "in" something, and contact,

ἅπτεσθαι

, are elucidated

[95] Apart from Owen's own discussions, noted above, n. 67, see also J. Barnes 1971, Hamlyn 1977–78, Tarán 1978, Ferejohn 1980, Irwin 1980–81, Fine 1982.

[96] *Metaph.* 1003a34ff., cf. *Top.* 106b33ff., *Metaph.* 1060b37ff. See, for example, Owen 1960/1986, pp. 192ff., 198ff., 1965a/1986, pp. 259ff., and cf., e.g., Mackinnon 1965.

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in such a way.**[97]** It is characteristic of his discussion to move outwards from a central, familiar, unpuzzling usage, gradually widening the range of what is to be included under the original rubric.

He proceeds in a similar way when clarifying the concepts of matter and form, and potentiality and actuality, for example, via the notion of proportional analogy, though this is not a *kind* of focal meaning so much as an alternative to it.**[98]** Thus matter—where the term he coined,

ὕλη

, originally meant just wood, of course—is used first of the stuff physical substances are made of, but also of the substratum of change more generally.**[99]** That there *is* something that underlies and survives change is illustrated by such straightforward cases as a man becoming pale or educated, but the idea is then applied not just to the bronze the statue is made from, but also more problematically, in embryology, to the matter—the menses—that Aristotle holds to be supplied by the mother to the embryo.**[100]** Again, matter is said to individuate members of the same species, which are the same in form but numerically distinct,**[101]** and he feels entitled also to speak of intelligible matter in, for example, mathematics.**[102]** Two identical triangles used in a geometric proof are

differentiated by their intelligible matter: not the triangles I draw on the blackboard, but the triangles we have specified and are reasoning about. As matter is what is characterised by form, and the genus receives determination from the species (also

εἶδος

), the genus too can be called matter.**[103]** But as that last example particularly illus-

[98] As becomes clear at *EN* 1096b26ff., for example, when he discusses "good."

[99] As, for example, in *Ph.* 1.6ff., 189a11ff., especially 7, 191a7ff., 9, 192a3ff.

[100] See, for example, *GA* 727b31ff., 729a10ff., 28ff.; cf. G. E. R. Lloyd 1983a, p. 97.

[101] *Metaph.* 1034a7f.

[102] See, for example, *Metaph.* 1036a9f., 1037a4ff., 1045a33ff.; cf. 1059b14ff., 1061a28ff., and cf. Lear 1982, p. 181.

[103] See especially *Metaph.* 1024b4ff. (but cf.b9ff.), 1045a34f., 1058a21ff., cf. 1016a24ff., 1023b2, 1038a5ff., 1071a36ff., *Ph.* 200b7f., *GC* 324b6ff.

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trates—where whether the genus *is* matter, or is just *like* matter, is disputed**[104]** —the point at which Aristotle is using the term in an "as if" way (that is, in a way *he* has to recognise as such) may be quite unclear and controversial.

Thus in a variety of contexts, dealing especially with the fundamental notions that underpin the whole of his philosophy and science, Aristotle offers a kind or kinds of analysis that while certainly not in direct contradiction to anything in his logic, nevertheless represent a certain relaxation of the requirements of univocity and universal, *perse* , predication laid down in his accounts of definition and demonstration.**[105]** The balance between these two points is delicate—the more so as we do not have an extended formal discussion of focal meaning and so have to rely on the

scattered comments that occur in texts that deploy the notion. But clearly, first, there is no question of Aristotelian *metaphora* being involved, in the sense of the transference of a term from one field to another. Nor, secondly, is focal meaning a matter of a comparison to be justified by reference to an (in principle) independently verifiable general rule exemplified in the particular cases compared. Thirdly, while the extent to which focal meaning is proposed by Aristotle as a *tertium quid* between what he calls synonymy (i.e., univocity) and homonymy is disputed,**[106]** at the very least it is marked out

[104] See, for example, Balme 1962a; A. C. Lloyd 1962, 1970; Wieland 1960–61/1975, pp. 136f.; Rorty 1973, 1974; Grene 1974; M. J. White 1975; Lear 1982, p. 181; I. Mueller 1987.

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from other cases of homonymy in that a systematic relationship can be exhibited between primary and peripheral significances. Nevertheless, fourthly, despite his evident dislike of some modes of reasoning based on likenesses, and despite the demand for the strictest univocity in all terms used in demonstrative reasoning, focal meaning and proportional analogy tacitly mark a departure from that ideal in many key concepts. This is not the reintroduction of imagery, but it is a loosening of the straightjacket of univocity, an implicit recognition (maybe) that the requirements specified for definition and predication in the *Posterior Analytics are* an *ideal* .**[107]**

The tension here mirrors and indeed exemplifies a further deep-seated tension within Aristotle's divergent statements on the relationship between philosophy and dialectic. Often that relationship is expressed in terms of a series of contrasts: the philosopher works—or can work—on his own, the dialectician in conjunction with a partner;**[108]** the philosopher deals with truth, the dialectician with opinion—for dialectical syllogisms reason from generally accepted views, demonstrative ones from premises that are true and primary.**[109]** Yet on other, admittedly rarer occasions, Aristotle recognises a fundamental role for dialectic, in the sense of the critical scrutiny of received opinions, as a means of securing the primary principles of each science.**[110]** But the snag is that the primary principles used in demonstrations, including definitions, are required to be better known than and prior to the conclusions.**[111]** To get round the difficulty Aristotle would no doubt invoke his distinction between what is "better known to us" and what is "better known simpliciter,"**[112]** but quite how the move from the first to the second is to be made, or how we are to recognise we have accomplished it when we have accomplished it, can be problematic**[113]** —

[108] See, for example, *Top.* 155b7ff.

[109] See, for example, *Top.* 100a18ff., 105b30f., *Metaph.* 1004b17ff.; cf. *Rh.* 1355a33ff.

[111] As at *APo.* 1.2.71b20ff., referred to above, n. 105.

[112] As at *APo.* 1.2.71b33ff.; cf. also *Top.* 142a6ff., *Ph.* 184a16ff., *Metaph.* 1029b3ff., *EN* 1095b2ff.

[113] See, for example, Wieland 1962/1970, pp. 69ff.; Mignucci 1975, pp. 30f.; S. Mansion 1979.

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particularly when we are dealing with cases that may involve focal meaning or proportional analogy. In any event the chief point that remains is that what is presented in the *Posterior Analytics* has to be seen as an ideal to which no more than approximations are to be expected in some key areas of inquiry.

Metaphor and the Development of Technical Terminology

I have so far focused largely on the evidence in Aristotle, since this offers by far the best opportunity to assess the match and mismatch between theoretical analysis and actual practice. But we should now extend the scope of our discussion to consider (once again, very selectively) some other aspects of the development and use of theoretical terms in Greek science more generally. Initially much of the inquiry into nature is almost entirely devoid of established technical terms: its discourse just *is* ordinary language and reflects—for better and for worse—its vaguenesses and unclarities. This is especially true of early Greek medicine, which takes over, more or less without modification, many popular terms for diseases.**[114]** The usual generic word for fever,

πυρετός

, which may simply mean fiery heat or fire, like

πῦρ

, in Homer,**[115]** is an example where the original associations of the term stayed

with it. Whatever theory a given Hippocratic author might adopt on the causes of diseases,

πυρετός

remained semantically wedded to the idea of heat. So too did

καῦσος

, from

καίω

, burn, though this was rather more a term of art: it was often connected with a combination of symptoms, even though these were neither distinct enough nor sufficiently

[114] Cf. Lonie 1983, pp. 152ff., who remarks on the comparative exiguousness of the Greek lexicon of disease names, with the exception of names for skin diseases and some other products of specialized medical knowledge; cf. also Kudlien 1967.

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widely agreed upon to justify the gloss in Liddell Scott Jones as "bilious remittent fever.**[116]**

The referential opacity of many popular, *and* newly coined, terms is particularly evident in words for diseases based on particular organs or parts of the body:

ὀφθαλμίη, πλευρῖτις, νεφρῖτις, ὑστερικά

.**[117]** The general sense was given by the root, in each case, though by itself this was not necessarily very informative. What counted as *the* disease of the pleura, or kidneys, let alone the uterus, was often a matter of dispute and depended on the writer's views on both the symptoms and the causes at work, though in some cases there were discernible limits to the sense and reference of the term and general agreement, for instance, that

ὀφθαλμίη

was an inflammation accompanied by discharges from the eyes (though these might be "dry").**[118]**

Similar points apply to many common terms in physiology. Both the advantages and the disadvantages of considerable semantic stretch can be illustrated in such a term as

πέψις

, usually translated "concoction." Originally used of the ripening of fruit (one of the root meanings of

[118] As, for example, at *Aër.* 10, *CMG* 1.1.2.48.19.

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the verb),**[119]** then of cooking and digestion,**[120]** it came to be applied to a wide range of physiological processes (including the production of semen and its action on the menses, the hatching of eggs, the development of the embryo, and the formation of blood, fat, suet, milk, and residues such as urine),**[121]** as well as to various pathological changes, the formation of pus, catarrh, phlegm, and other humours**[122]** (compare, in English, talk of the "ripening" of boils). Finally

πέψις

was even used of the production of metals and stones from earth,**[123]** and of snow, hoarfrost, and hail from rain.**[124]**

[121] For some representative texts in Aristotle, see *PA* 652a9f., *GA* 719a32ff., b2, 727a34ff., 744b1ff., 753a18ff., 756b28f., 775a17f., 776a20ff., b33ff., 780b6ff., *Mete.* 4.2.380a1ff., and cf. many passages that distinguish male and female by the capacity/incapacity to concoct semen, e.g., *GA* 728a18ff., 738a13, 34ff., 765b10ff., 766a30ff.

[123] As, it would appear, in the pseudo-Aristotelian *De plantis* 822a25ff.; for ancient beliefs concerning the *growth* of minerals see, for example, Halleux 1974, pp. 67 and 152, citing especially Proclus *In Ti.* 1.43.1ff.

This is a particularly clear example illustrating the difficulties of erecting *definite* boundaries between primary and derivative uses: there is no question of saying precisely where the term begins to be applied "metaphorically." As a portmanteau concept, it *both* enables a variety of different processes to be related and brought under the scope of a single theory, *and* it pays a price for this in the indeterminacy of the theory and a corresponding lack, at many points, of predictive or explanatory power.

But while large areas of Greek science, at every period, manifest a certain conceptual vagueness, there are important exceptions to this, cases where technical terms are coined and given clear working definitions. Anatomy, zoology,**[125]** harmonics,**[126]** and astronomy all provide examples, but we may concentrate on some from the first and the last of these. In anatomy, for instance, once some Greek investigators had begun to use dissection extensively,**[127]** many structures were discovered

[127] The debate within the Hellenistic medical sects, relating in part to human dissection and vivisection, is discussed above, Chap. 3 at nn. 220–235. But before that Aristotle had both advocated and practised animal dissection (see, e.g., *PA* 645a26ff.), though the extent of dissection before him is controversial: see G. E. R. Lloyd 1975a, Manuli and Vegetti 1977, Vegetti 1979.

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that had no popular names. Often the new coinages were constructed on the basis of analogies of one kind or another. What we call the retina, for example, was dubbed the net-like membrane (

ἀμφιβληστροειδής

in Greek: the origin of our own term, via the Latin intermediary, *rete* , net) though it was also called the spider's-web-like one (

ἀραχνοειδής

).**[128]** This illustrates one recurrent problem, namely, the standardisation of anatomical terms or, rather, the lack of it.**[129]** But whichever term was used, the associations inherent in the original comparison were unlikely to prove problematic, at least when the reference was clearly fixed. Even where the assumptions implicit in the new coinage bore a theoretical load, provided that the reference was definite, the term sometimes continued in use even after the theory changed. Thus the carotid arteries (

καρωτίδες

, stupefiers) were so called because originally they were believed to cause unconsciousness when pressed; but even after it was established that this was an effect of compressing the nerves in the neck rather than the arteries, the term was still used of the arteries in question.**[130]**

Finally, astronomy, especially, developed a wealth of technical terminology, clearly defined words for zenith, meridian, apogee, perigee, parallax, colure, station, retrogradation and many others,**[131]** let alone geometrical terms such as homocentric, epicycle, eccentric. Where imagery continued to play a more prominent and indeterminate part is, rather, in the sister discipline of astrology, where conclusions were drawn about the influences of planets and constellations from their

[129] Aspects of this are discussed in G. E. R. Lloyd 1983a, pp. 158ff.

[130] See, for example, Rufus *Onom.* 163.9ff.; cf. Galen *PHP* 1.7, *CMG* 5.4.1.2.86.24ff., (K) 5.195.4ff.; *UP* 16.12 (H) 2.427.15ff., (K) 4.332.9ff.

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supposed masculine and feminine qualities, their commanding or obedient character, their being diurnal or nocturnal, beneficent or maleficent.**[132]**

The Category of Metaphor and the Criteria for Truth

A comprehensive study of "metaphor" in Greek science would be a comprehensive study of Greek science. But perhaps enough examples have been passed very rapidly under review to allow a certain perspective on the range of uses and to permit us to take stock of aspects of the polemic that some Greek philosophers directed against metaphor, myth, and other modes of reasoning involving likenesses or the non-univocal.

As we should expect, the Greek inquiry into nature is heavily dependent, in all periods, on every kind of more or less evidently stretched terms. Even those who might not accept that all language is in some sense metaphorical, will agree that it is often, even normally, through the generation of new, and the elaboration of old, models that science grows and acquires new ideas—and this is true, for sure, of ancient inquiries. The conceptions of concoction, of organ, and of matter itself provide examples, at different theoretical levels, of ingenious and surprisingly durable cases of creative semantic stretch from Greek science, even if we must grant that elsewhere such uses also permitted much merely wayward speculation.

[132] The use of these terms can be extensively illustrated from Ptolemy's

Tetrabiblos (which deals with masculine and feminine planets and signs at 1.6.20.8ff. and 13.34.9ff., with diurnal and nocturnal planets at 7.21.2ff., commanding and obeying signs at 15.37.3ff., and beneficent and maleficent planets at 5.19.17ff., developing the characterisations of 4.17.13ff.), as also can elaborate symbolism drawing on the animal or human forms and characters of the signs of the zodiac and other constellations (especially in 2.8.81.5ff., 82.15ff.). At the same time, astrology also possessed a technical vocabulary for certain geometrical relations (Ptolemy explains "trine"—triangular—"quartile," and "sextile" in 1.14.35.20ff.), as well as sharing with astronomy the terminology of zenith, meridian, ascension, syzygy, and so on.

Strong reservations are expressed first by Plato and then by Aristotle on the use of images, likenesses, myths, and metaphors, and the aggressiveness of their tone is a pointer to that underlying polemic. At the very earliest stages of the inquiry into nature, what it was replacing or attempting to replace was a view of the world put together (but not consciously) from elements relating to straightforward, concrete experience, and applied (again, not consciously) to the understanding of the otherwise inexplicable. The unexpected, the imaginary, the frightening, the occult, can only be comprehended within a coherent network of beliefs by some such extrapolation from the domain of the known, the familiar, the unproblematic—though that way of putting it runs the risk of representing the spiritual world as less well known, or at least less vividly apprehended in belief, than the world of tables and chairs, and that may well not be the case at all.**[133]**

The question of the status of ideas applied across a variety of contexts (as it might be, to the gods as well as to humans) was not an explicit issue until the philosophers made it such, until they made problematic the whole question of what *counts* as a *variety* of contexts. The effect of having an explicit category of the metaphorical was that it enabled issues of meaning and commitment to be brought into the open and, indeed, to be pressed—and the nature of the challenge to which the whole corpus of traditional beliefs might be subject was thereby transformed. It is particularly striking, in view of the way in which poetry was later viewed by some philosophy, that the fifth-century poets themselves, Pindar and the tragedians especially, can be seen as already frequently raising—more or less directly—problems concerning naming, meaning, understanding, and deception.**[134]** Yet it was not their concern, of course, to develop *explicit* theories to do with the relationship between language and reality. The special *sophia* they often claimed was—naturally—heavily dependent on what Aristotle would

[133] The vividness with which the spirit world may be apprehended is well conveyed in such studies as Lambek's of the Mayotte: Lambek 1981.

[134] These are especially prominent themes in the *Oresteia* , as has recently been shown in detail by Goldhill 1984.

deem metaphor, when it was not also deliberately enigmatic.**[135]** Above all, lacking an *explicit* concept of the metaphorical, they lacked also one of the literal, and the idea of tying the notion of truth to the latter would no doubt have seemed, to a Pindar, bizarre in the extreme.

Aristotle's invention of the metaphorical/literal dichotomy involved the stipulation of criteria for truth that at one stroke downgraded—even ruled out—poetry, most traditional wisdom, and even much of earlier philosophy. In his view most pre- or proto-cosmological thought is open to the charge of being *mere* "poetic metaphor," and where "metaphor" was diagnosed, the question of what the metaphor was a metaphor *for* was one that could always be pressed. When he separates

φυσιολογία

from poetry, this may be seen as part of a continuing argument to define and legitimate the former domain.**[136]** What he places to the side of poetry is thereby excluded from the concerns of the natural philosopher; what is put to the side of rhetoric is all very well for the aims of persuasion, but formal logic and demonstration demand stability of sense and reference and so the exclusion of whatever threatens that stability. At the limit that stability is the ideal that not just mathematics but philosophy, including natural philosophy, should *aim* at—though, as I noted before, Aristotle clearly recognises and indeed insists that the physicist deals with what is true for the most part as well as with what is true always, and that physics is not and cannot be as exact as mathematics,**[137]** as, indeed, his own practice in the physical treatises, steeped in dialectic, bears out.

The polemic against metaphor and myth is thus part of the campaign waged by philosophy and science against poetry and religion—or at least against some traditional religious beliefs—although of

[135] Cf. above, Chap. 2, pp. 85f. and n. 134.

[137] See above, Chap. 3 at nn. 136ff.

course the ancient forms of those battles must not be conflated with those they have taken since the advent of Christianity, let alone more recently. At the most general level, however, the ancient battles too related to the erection of boundaries marking the spheres of influence and the domains of discourse of those with varying claims to intellectual leadership and prestige.

To be sure, once the barriers were up, there were those who were prepared to try to exploit the possibilities offered by the category of the metaphorical to resist a certain type of challenge concerning the *literal* truth of their pronouncements, even though quite what claims they *were* making sometimes remained quite unclear. As can be illustrated already from within the texts of Plato himself, the category of myth could be used, on the one hand, to condemn the ideas so characterised but, on the other, to insulate a viewpoint from a certain type of criticism.**[138]** Yet once the Aristotelian dichotomy between the literal and the metaphorical was available, to plead the defence of the metaphorical by claiming that a term was used *just as* a metaphor would by itself hardly do, for *without overhauling and criticising that dichotomy itself* such a defence was likely to seem merely evasive, leaving unanswered the Aristotelian demand for the literal account of which the metaphor was a metaphor. Meanwhile, on the side of the literal, the forging of that dichotomy was certainly one manifestation of Aristotle's confidence that literal truth was there to be attained, and while that was usually, no doubt, a sign of a certain simplistic overconfidence on his part, it provided, we may be sure, a powerful incentive to the pursuit of his style of scientific investigation.

Nor were the boundaries that were set up just a matter of sociology, to be accounted for by externalist considerations to do with the rivalry between different groups competing for prestige. Although we should now say that the differences between poetic, and scientific, discourse are less, and less clear-cut, than Aristotle, for one, often sought to in-

[138] These topics have been elaborated by Detienne 1981/1986. The positive aspects of Plato's use of myth have been well brought out by Ferrari forthcoming.

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sist, the constraints on language in the two areas are not *identical* . In particular, the natural scientist is bound to have to explore, and at points to delimit, the implications of his "metaphors" with one eye on the need for his theories to come eventually (and no doubt not one by one)**[139]** to a distinctive type of empirical or pragmatic test. The contrast with the way in which poetry works or is effective is obvious. So too, to take another field to make an analogous point, in law the lawyer will need to define and clarify his terms, though, again, without hoping for *complete* precision: Aristotle could be used to illustrate the recognition of the need,**[140]** Lysias the resistance

to a demand for precision.**[141]**

So far as the sphere of the understanding of nature goes, the positive features of the conceptual moves we have been studying (and it is important to recognise that there *are* some positive features) may be said to lie in the favouring of the explicit over the merely allusive, of *comparative* determinacy over interdeterminacy, obscurity, even fudge. Suggestive though it may be to view sleep as poured over you, or to speak of the "channels" of communication as just that,

πόροι

, within the body, such ideas cannot be scrutinised or made the subject of further investigation—at least not until the limits of the commitment of the implied conceptions are made clearer. To be fair, however, even when the term

νεῦρον

had been (partially) disambiguated and the sensory and the motor nerves distinguished and made the topic of detailed research—as by Herophilus and Galen—the stretch of that term still permitted continuing indeterminacies,**[142]** and theories about the *mecha* -

[139] See, for example, Hesse 1974, chap. 2, 1980, chap. 3.

[140] In *Rh.* 1.13.1373b38ff., Aristotle discusses cases where the characterisation of what was done is in dispute, where the issues turn on the distinctions between, for example, "theft" and "sacrilege," "intercourse" and "adultery," "dealing with the enemy" and "treason" (1374a3ff., 6ff.). Cf. also Plato *Lg.* 943e–944c.

[141] That the law does not attempt to specify all the terms that fall under a general rubric or have the same general sense is asserted by Lysias at 10.6ff., though elsewhere often—and unsurprisingly—enough his argument depends on insisting on distinctions between types of case assimilated or confused by his adversaries, e.g., 3.41ff., 4.9.

nisms of the transmission of movement and sensation (as opposed to descriptions of the courses of the nerves)**[143]** remained extremely vague. There was a long way to go.

But the—more obvious—negative features of this polemic include especially the rejection of, or at least lack of sympathy for, much that was heuristically fruitful in imagery, analogy, myth. Nor were these three by any means always characterised by—and they certainly had no monopoly of—obscurity, fudge, and the allusive. Moreover, the expectations of transparency and univocity that the philosophers generated were not ones they were usually or even often in a position to fulfil, even though they were, up to a point at least, useful expectations to raise, if only to focus on the ideal or the limiting case. The vocabulary of *muthos* and *logos* and of metaphor enabled a distinct type of challenge to be pressed, that of specifying the limits of the commitment to a theory, even though, maybe, in the strictest versions of the tests, with the emphasis on the *precision* of the limits, they were ones that science itself (then and now) was bound finally to fail—at least if it continues to use natural language or, rather, because it has to do so.

Some of the most outspoken condemnations of metaphor come in the context of Aristotle's discussion of the conditions for *episteme* , understanding, in the *Posterior Analytics* . Only universal, per se, predications using univocal terms will do for the purposes of demon-

stration, and accordingly there is no way to make room for *metaphorai* here. Yet to have confined the scientist to what would meet the requirements for demonstration according to the official programme in the *Analytics* would have been massively restrictive. Nor is there any question of Aristotle in practice actually so limiting the domain of physics. In fact, however, when, under the influence of that programme or not, he attempts precise definitions of complex concepts, the result is sometimes a certain arbitrariness (as we saw with his definitions of the four primary opposites). More often he is more tentative as he moves towards the delineation of fundamental concepts, especially when he uses the notion of focal meaning in their explication, even though that notion remains (we said) in a somewhat anomalous position in relation to the official programme.

The programme is a part, in a way the culmination, of that aggressive attack mounted by the new wisdom, at this point, against the old (and against some rivals from among the new) in a bid to supplant them. Yet once again, in practice, the new had, and continued to have, more in common with the old than the form that the attack took might lead one to expect. Certainly the effect of the forging of an explicit terminology to distinguish different types of discourse and different claims for truth was to raise the most radical questions with the most far-reaching repercussions, reverberating to the present day. Yet although Aristotle's desideratum for the well-ordered presentation of a mature science is that it should limit itself to strictly univocal terms, aspects of the actual science he does are evidently a good deal less rigorous—to their advantage. While there is no way in which he will allow *poetry* back into *science* ,**[144]** in acknowledging the role of dialectic

he recognises that the language of the scientist will often remain some distance from the ideal formalisations of the *Analytics* .

[144] This remains true even though, in the *Poetics* , reacting perhaps in part to Plato's criticisms and his exclusion of most poetry from the ideal state in the *Republic* , Aristotle restores to poetry in general and to tragedy in particular an honourable place both in its educative role and as a source of pleasure: see especially *Po.* 4.1448b4ff.

Chapter Five— Measurement and Mystification

One radical criticism that has been levelled at ancient Greek science is that it was essentially qualitative in character. In both the physical and the life sciences, so it has been said, theories were neither given exact quantitative expression—as in modern chemical formulae—nor supported by exact quantitative data. One influential proponent of this thesis, who saw this as the distinguishing characteristic of ancient science, indeed, of all science up to Galileo—and a characteristic that seriously diminished its claims to *be* science—was Alexandre Koyré;**[1]** his view has been endorsed, and the thesis further elaborated independently, by several other prominent historians of science, including Temkin, Kuhn, and Joly.**[2]** In his paper entitled "From the World of the Approximate to the Universe of Precision," Koyré argued that the Greeks had no real technology, no real physics in our sense. No attempt was made to mathematise terrestrial physics; indeed, ancient science "never attempted to use on earth a measuring instrument or even to measure exactly anything except distances."**[3]** Referring to the whole of science

[1] Koyré 1948/1961, pp. 311ff., and 1968, especially pp. 89ff.

[2] Temkin 1961; Kuhn 1961/1977; R. Joly 1966, pp. 108ff. Cf. also Aaboe and Price 1964.

[3] Koyré 1948/1961, p. 313: "Elle [i.e., Greek science] n'a jamais essayé . . . d'employer sur la terre un instrument de mesure et même de mesurer exactement quoi que ce soit en dehors des distances." In a footnote Koyré remarks that Vitruvius' description of a theodolite for measuring horizontal and vertical angles constitutes an exception and that precious metals were weighedprecisely. The thesis that the Greeks did not and could not develop a real technology and lacked physics in the modern sense is propounded at pp. 311ff.; cf. also 1968, pp. 22ff., 34ff.

before Galileo, he put it that "no one had the idea of counting, of weighing and of measuring. Or, more exactly, no one ever sought to get beyond the practical uses of number, weight, measure in the imprecision of everyday life."**[4]**

Koyré's studies had the great merit of focusing on a fundamental issue that goes to the heart of the question of the quality of much ancient scientific work, and the thesis he propounds is bold and simple. Among the few qualifications he enters are the success of Greek celestial physics: the lack of physics is a lack of sublunary physics. Moreover, even so far as sublunary physics goes, the "superhuman" "divus" Archimedes, as he calls him, receives honorific mention, although the limitations of his achievement (a statics, but no dynamics) are stressed.**[5]** As those exceptions indicate, Koyré no doubt realised that the problem is more complex than some of his generalisations might seem to allow. In what has inevitably to be a highly selective discussion here I shall try to do three things: first, to see how far Koyré was right about a systematic failure to use measurement in ancient science; second, to illustrate some of the negative as well as the positive features of the ancients' search for exactness; and, third, to review briefly some of the underlying epistemological presuppositions at work. That is, in what contexts and in what forms did the ancients seek or demand exactness or even believe it to be possible? How far did they have a concept of measurement? As in our earlier studies, we shall try to evaluate not just the principles the ancients adopted but also the match between principles and practice, and we must be prepared to recognise once again that the complexity and ambivalence of ancient presuppositions and practice are such as to make generalisation hazardous.

[4] Koyré 1948/1961, p. 318: "Personne ne s'est avisé de compter, de peser et de mesurer. Ou, plus exactement, personne n'a jamais cherché à dépasser l'usage pratique du nombre, du poids, de la mesure dans l'imprécision de la vie quotidienne."

[5] See Koyré 1968, pp. 14, 22, 32, and especially 38.

We may consider first two familiar fields that appear to provide strong evidence for Koyré's thesis: dynamics (his star example) and element theory.

Dynamics

In the field we call dynamics, the study of moving bodies, it is well known that up until Aristotle the Greeks had not progressed much beyond general statements of such vague principles as that "like" seeks "like," a principle applied by the fifth-century atomists, for example, to animate as well as to inanimate phenomena, to birds as well as to pebbles on a beach.**[6]** Notoriously, Aristotle himself attempts no *theory* of the factors influencing the speed of moving bodies, whether in "natural" or in "forced" motion, but merely introduces a number of general statements on that topic in the course of his discussion of other problems, such as the existence of the void or that of an infinite body, in the *Physics* and *De caelo* .**[7]** Many of these statements are quite indeterminate. For example, several suggest merely that in natural motion the more there is of a heavy body the faster it moves downwards, and, similarly, that the more there is of a light body, such as fire, the faster it moves upwards. Thus at *Cael.* 277b3ff., for instance, in the course of an argument denying that the natural movement of the elements is due to an external force, he says simply that "it is, on the contrary, always

[6] Democritus fr. 164, and cf. Aetius 4.19.3. Furley 1976, p. 85, suggests, however, that the example of the birds may be meant purely "to illustrate that there are instances of natural sorting without the action of a discriminating mind." Cf. C. W. Müller 1965a, pp. 76ff. On early atomistic accounts of motion in general, see Furley 1967 and 1976, O'Brien 1977 and 1981.

[7] There is an incisive discussion of this topic in Owen 1986a, pp. 315ff., following Owen 1970/1986, with powerful criticisms of such earlier studies as Drabkin 1938. Cf. also Carteron 1923, Cornford 1931, W. D. Ross 1936. I would now want to qualify some of the views I expressed in G. E. R. Lloyd 1964 and cf. 1968b, pp. 175ff. Among other important recent discussions, apart from Owen's, see de Gandt 1982; Hussey 1983, additional note B, pp. 185ff.; and Wardy 1985.

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the case that the more fire there is the faster it moves, and the more earth likewise to its own place."**[8]**

Sometimes, however, he refers to certain proportions. Thus in natural motion he suggests that the increase in speed is "according to the ratio" of the increase in the weight or the impulse (

ῥοπή

) the body has. At *Ph.* 216a13ff., for instance, he says: "we see that bodies that have a greater impulse either of heaviness or of lightness, if they are alike in other respects,**[9]** move faster over an equal space and according to the ratio that their magnitudes have to one another."**[10]** Again, in *Cael.* 273b30ff. he puts it: "if a certain weight move a certain distance in a certain time, a weight that is greater**[11]** will move [the same distance] in a less time, and the proportion that the weights have [to one another], the times too will bear conversely; for example, if the half weight [covers the distance] in such a time, double that amount will do so in half that time."**[12]**

[11] Literally, "which is so great and more."

The principal problem of interpretation that these texts pose is the extent to which Aristotle should be seen as committing himself to precise general laws of natural motion stating a relationship of the form V *μ* W.**[13]** The references to proportions have often been taken, in ancient and in modern times, to imply this. Yet as Owen and others have shown, that is in certain respects clearly, and in others very probably, an overinterpretation. First and foremost, Aristotle's interest in these passages is not in the factors governing the speed of naturally moving bodies at all. Rather, he is concerned to develop arguments in the first passage to disprove the existence of the void, and in the second to refute the possibility of an infinite body with infinite weight. His argument, in both cases, rests on the fact that there is *no* proportion between a *finite* magnitude and an *infinite* one,**[14]** or between a finite magnitude and *zero* ,**[15]** and so all he needs is to point out that there is *a* proportion, *some* finite relationship, between speeds or times and "impulses" or weights. Not only does he *not* state a formula of the form V *μ* W, he shows no interest in describing clearly under what conditions the increase in speed he refers to takes place. *Some* increase with weight/lightness is taken as common knowledge,**[16]** and no attempt is made to specify this exactly, let alone to determine it by measurement.

Further references to proportionalities occur also in some of Aristotle's remarks concerning "forced" or "artificial" motion, and here, even more clearly, a weak interpretation is preferable—indeed, at

[13] That is, in the same medium: Hahm 1976, p. 77, is one of many who ascribe this "fateful error" to Aristotle. Similarly, on such a line of interpretation *Ph.* 215a31ff. would be taken to suggest that velocity is inversely proportional to the density of the medium—however that is to be evaluated.

[14] As at *Cael.* 274a7ff.

[15] As at *Ph.* 215b12ff.

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points, required. Thus in *Physics* book 7 chapter 5, especially, he considers the effects of "powers" of different strengths on the speed of the objects they move, where he has in mind, among other things, such familiar cases as a gang of men hauling a ship.**[17]** At *Ph.* 249b30ff. he says: "if A, which causes the motion, moves B a distance C in a time D, then in the same time the same power A will move half B twice the distance C, and in half the time D it will move it [half B] the distance C. For thus it will be proportional. And if the same power move the same object a certain distance in a certain time and half the distance in half the time,**[18]** then half the strength will move half the object an equal distance in an equal time; for example, let E be half the power A, and F half B, then the proportion of the strength to the weight will be the same, and so they will move an equal distance in an equal time."**[19]**

Thus far it might appear that we have here the makings of exact general laws of forced motion. Yet Aristotle immediately proceeds, at *Ph.* 250a9ff., with a statement that qualifies the field of application of the principles he has just sketched out. "And if E moves F a distance C in a time D, it is not necessary that E moves twice F half the distance C in the same time. If indeed A moves B a distance C in a time D, half A—that is, E—will not, in a time D or in any fraction of it, move B a part of the distance C which is in the same proportion to it as A is

[17] See *Ph.* 250a17ff., cited below, and cf. *Ph.* 253b18.

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to E. For it may be that it will not move it at all."**[20]** Otherwise, as he goes on to say, "one man might move a ship, since both the strength of the haulers and the distance they all cause the ship to move are divisible by the number [of the men]."**[21]**

This exception strongly suggests that the proportionalities adumbrated in this chapter are not intended to apply *strictly* as *universal rules* .**[22]** This becomes clearer still when we consider the range of phenomena that Aristotle believes to exemplify such proportionalities.**[23]***Ph.* 250a28ff. states that growth or increase, and even qualitative changes, are generally subject to similar rules, for example, that "in

[22] On attempts to reconcile the proportionalities of *Ph.* 7.5 with Newtonian dynamics, see especially Owen 1970/1986, pp. 156f. (compare Hussey 1983, pp. 196ff.). This book has the quite general task of arguing for a first *moved* mover by showing that an infinite sequence of movements is impossible. On the assumption (which he would not share) that celestial movements may be deemed to be such a sequence of *artificial* movements (though for Aristotle, of course, they are natural), chapter 5 would provide an argument to show that infinite artificial motion would have to be powered by an infinite mover (cf. the arguments in *Cael.* 273b30ff., considered above, and *Ph.* 8.10.266a10ff., which rule out an infinite body and an infinite magnitude). It must be acknowledged, however, that there is no explicit direct application of the analysis in chapter 5 to the general concerns of the book, and the question of its place in the overall strategy of that book and of the *Physics* as a whole remains problematic (cf. Manuwald 1971). Nevertheless, as Wardy (1985) has recently argued with some force, the ambition to find *general* laws of motion in this chapter founders not only (1) on the absence of any explicit distinction between natural and artificial motion, but also (2) on the inapplicability of the proportions to cases of artificial motion where the speed is not constant (e.g., the motion of projectiles), and (3) on the failure to specify clearly the limiting case marking out the exceptions that Aristotle allows.

[23] The point was again made emphatically by Owen 1970/1986, pp. 156f.

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twice the time, twice as much alteration will take place."**[24]** Again, in the *De caelo* , 274b34ff., when he proposes a similar analysis for a variety of modes of change, he begins by specifying "heating" and "pushing" and then generalises his point to apply to any affection or movement whatsoever. If we bear in mind Aristotle's essentially qualitative conception of the hot/cold spectrum, there is clearly no question of the proportions or ratios in such a case being expressible in exact quantities.

For anyone on the lookout for the first signs of an ambition to arrive at strict quantitative laws of motion, Aristotle's statements about how objects move, in natural or forced motion, are a disappointment. But then Aristotle clearly had no such ambition. Certain proportionalities are stated that might, at first sight, be taken to be part of such a general theory, set out in the form of exact equations governing the speed of moving bodies. Yet these statements are generally made *en passant* in the course of his discussion of other topics, where he has dialectical, often destructive, ends in view and is certainly not concerned with the positive development of any exact general theory of motion—and where the statements in question are intended to apply only loosely or subject to exceptions which are themselves not specified precisely.**[25]**

Aristotle's views were influential, but he was, of course, far from being the only theorist to discuss aspects of the problem of motion. Several later writers implicitly or explicitly contested his statements, and some certainly made some attempt to broaden the empirical base of the discussion. A passage in Simplicius shows that the third head of the Lyceum, Strato, tried to adduce evidence for the phenomenon of acceleration (as we call it) in natural movement. Yet as reported by Simplicius, at least, Strato's observations are typically imprecise: "If one observes water pouring down from a roof and falling *from a con -*

[24] *Ph.* 250a28ff., b2. Again, the exception is noted, at b4ff., that it is not necessarily the case that if what causes the alteration is halved, half the alteration is brought about. Cf. *Ph.* 253b13ff., where discontinuities in increase and diminution and in other modes of change are accepted and indeed insisted upon.

[25] Cf. above, n. 22, on *Ph.* 7.5.

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siderable height , the flow at the top is seen to be continuous, but the water at the bottom falls to the ground in discontinuous parts. This would never happen to it unless it traversed each successive space more swiftly."**[26]** Again: "if one drops a stone or any other weight *from a height above the earth of about a finger's breadth* , the blow made on the ground will not be perceptible, but if one drops the object *from a height of a hundred feet or more* , the blow it will make will be a powerful one."**[27]**

Much later, in the sixth century A.D. , Philoponus, the most devastating ancient critic of Aristotle's views on dynamics, certainly sought to refute those views by appeal to what he represents as empirical as well as to logical considerations.**[28]** He takes Aristotle's doctrine of natural motion to imply that in motion through the same medium, the times required for the movement will be inversely proportional to the

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weights or impulses of the moving bodies.**[29]** To this he then comments: "This is completely false, and this can be established by what is manifestly evident more powerfully than by any sort of demonstration by arguments."**[30]** He envisages the possibility of a test to refute Aristotle's theory, and yet the imprecision of this, as he goes on to describe it, is remarkable. "For if you let fall at the same time from the same height *two*

weights that differ by a very large measure , you will see that the proportion of the times of the motions does not correspond to the proportion of the weights, but that the difference in the times *is a very small one* . So if the weights were not to differ by a very large measure, but the one, for example, were to be double the other, there will be no difference in the times of the movements, or if there is one, it will be imperceptible, although the difference in the weights is by no means such, but the one has the ratio of double the other."**[31]**

Although elsewhere in his discussion Philoponus occasionally refers to some specific weights, distances, and times for purely illustrative purposes,**[32]** no attempt is made to report *precise* results of actual tests.

[32] Thus he does so at *In Ph.* 683.12ff. to illustrate the implications of theprinciple he attributed to Aristotle. Cf. in other contexts also at *In Ph.* 646.22ff., 677.20ff., 681.17, 30ff.

Even in these texts, which offer probably the most sustained ancient discussion of dynamical problems, Philoponus is content to show quite generally that the proportions he takes to be implied in Aristotle's statements are wide of the mark—without recording the exact measurements obtained in a series of particular trials (if, indeed, he carried these out).**[33]**

Yet this should not be said to be just a matter of a conceptual block. A technical factor is certainly at work, though the importance we attach to it may be a matter of some disagreement. The association of movement and time with *number* is already found in Aristotle, who calls time "the number of motion in respect of before and after."**[34]** Yet in practice neither Aristotle nor anyone else in the ancient world had any means of exactly measuring short intervals of time.**[35]** The day was divided into hours of variable length, an hour being a proportion of daylight or darkness. Shorter periods were measured by such devices as the water clock or sundial. But even after Ctesibius had introduced an improved constant-flow water clock in the third century B.C. , accuracy in measuring short periods to within an interval corresponding

[33] Cf. also his discussion of Aristotle's view of the role of the density of the medium in natural motion at *In Ph.* 647.12ff. At 647.18ff. (cf. 682.30ff.) he represents Aristotle as asserting that the times of the movements will have the same ratio as the density of the media (though Aristotle had put the point hypothetically: see above, n. 10). Philoponus observes that this principle is difficult to refute, because of the difficulty of evaluating the difference in the density of the media (*In Ph.* 683.1ff.).

[34] *Ph.* 219b1f. (number here in the sense of what is counted, not that by which we count: cf. 219b5ff., 220b8ff.). Aristotle also points out that we not only measure movement by time, but also time by movement (*Ph.* 220b14ff., 23ff., 223b15ff.).

[35] Contrast the situation regarding the evaluation of long intervals of time such as astronomical periodicities. Estimates of the solar year and lunar month began, in Greece, in the mid-fifth century B.C. (cf. below at n. 72) and eventually Ptolemy, drawing on the work not just of earlier Greek astronomers but also of Babylonian ones going back, in some cases, to the eighth century B.C. , gave figures for the two main periodicities of each of the planets that are accurate to within 0.002 percent of the modern computed value in every case.

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to a second—or even to ten times that amount—was neither, usually,**[36]** sought, nor was it obtainable.**[37]**

The evaluation of this technical consideration is, as I noted, problematic. After all, some of Galileo's own experiments in the *Discorsi* involve timing devices that are not markedly superior to—some are even cruder than—Ctesibius' water clock.**[38]** Yet that it provides part, at least, of the answer to the lack of exact measurements in ancient terrestrial dynamics is surely clear, even if this technical failure to advance beyond Ctesibius itself poses a set of problems concerning ancient motivations and interests. We should certainly not ignore the fact that some attempts were made to bring empirical data to bear on dynamical problems in antiquity, even if not *precisely measured* data. But before agreeing to any wholesale conclusions about a lack of *any such* measurements in ancient science we have quite a number of other fields to consider.

Element Theory

The second example I mentioned as favourable, on the face of it, to Koyré, was element theory. What were the factors at work here? The dominant strand in ancient theories concerning the fundamental constituents of physical objects is represented by doctrines that refer to earth, water, air, and fire, or to hot, cold, dry, wet, and their like, whether singly or in combination, as the ultimate elements.**[39]** Admittedly, atomism in each of its ancient Greek forms**[40]** has a quantitative

[36] Cf., however, on Marcellinus' report concerning Herophilus, below at n. 230.

[37] The accuracy of ancient timing devices has been discussed by, for example, Fotheringham 1915 and 1923; Dicks 1953–54; Price 1957; Landels 1978, pp. 188ff., and 1979.

[38] This point was already made by Koyré 1948/1961, p. 327, 1968, pp. 93f.

[39] Among important recent general discussions of ancient element theory those of Solmsen 1960, Wieland 1962/1970, Seeck 1964, Mau 1969, Happ 1971, Vlastos 1975a, should be mentioned especially.

[40] The evidence for the atomic theory of Leucippus and Democritus in the fifth century is mostly indirect and comes from generally hostile sources.Plato's geometrical atomism, based on two types of elementary triangles, is set out in the *Timaeus* . For the Epicurean version of Democritean atomism we have the *Letter to Herodotus* , the *Letter to Pythocles* , fragments of the major work *On Nature* , and Lucretius. See Furley 1967, 1976, Vlastos 1975a, in the first instance especially.

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aspect. The fundamental indivisible particles are differentiated by shape, size, arrangement, and (sometimes) weight.**[41]** But though in part mathematical, no ancient atomic theory is anything but impressionistic in its application. It is not just that there are no strict correspondence rules, as in Hempel's schema, to get from initial hypotheses to observation statements; rather, the link between these two is left massively indeterminate. Precise measurements are not, in any case, normally deployed in such observation statements as are cited by way of illustration of the theories—which appeal, rather, to some fairly vague physical analogies.**[42]** It is striking, then, that when Aristotle criticises the atomists for failing to account for alteration and for the interaction of bodies, he is objecting in part to their attempts to reduce qualitative differences to quantitative ones.**[43]** Yet no ancient atomic theory succeeded in *deriving* the former from the latter.

Meanwhile in the predominant, qualitative element theories, such as Aristotle's own, opportunities to undertake direct measurements are rarely taken. Like the dry and the wet, the hot and the cold themselves are not, in any case, deemed to be capable of measurement. The gadgets described in Philo and Hero that are sometimes called thermoscopes fall well short of being measuring devices.**[44]** Certainly they

[41] On the issue of whether for Leucippus and Democritus the atoms have weight as an intrinsic property (as they certainly did later, in Epicurus' theory) see Furley 1976 and 1983b; Hahm 1976; and O'Brien 1981, pp. 330ff. (which includes a very full if often hypercritical review of earlier

scholarship).

[42] This applies both to the theories correlating perceptible qualities with atomic shapes that Theophrastus *Sens.* 49ff., 61ff., attributes to Democritus, and to those set out in Plato *Ti.* 53c ff., 55d ff., 57c ff.

[44] Philo *De ingeniis spiritualibus* (*Spir* .) 7 (474–76 Schmidt), Hero *Pneu -matica* 2.8 (224.2ff. Schmidt). In both cases Schmidt, in his edition, labelled the devices *thermoscopes* . Cf. A. G. Drachmann 1948, pp. 119ff.

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show that in some contexts, at least, it was recognised that gases expand with heat. In Philo, a sphere containing air and hermetically sealed is connected by a bent tube to a vessel containing water. When the sphere becomes hot "on being left in the sun" (though Philo notes that the same effect is obtained as well when the sphere is heated in other ways),**[45]** the air bubbles out of the vessel, and when the sphere cools, water is drawn back up the bent tube towards and into the sphere. Such an instrument *might* have been adapted to give rough measurements of temperature. Yet neither Philo nor Hero gives any hint that they appreciated this possible application. Their devices (like so many others described in their works) serve merely to illustrate a striking effect.**[46]** Again when in later writers, such as Galen, we encounter talk of *grades* or *degrees* of hot and cold, wet and dry,**[47]** this is no more than a *theoretical* elaboration; the grades are not thought to be measurable.**[48]**

[45] Philo *Spir.* 7.474.27ff.

[47] The theory of the different standards by which hot and cold, hotter and colder, are to be judged is developed in the *De temperamentis* (= *Mixt.*), for example 1.6.21.20ff. Helmreich, 1.542.13ff. Kuhn; 1.8 (H) 29.3ff., (K) 1.554.12ff.; 1.9 (H) 32.24ff., (K) 1.560.13ff., and the terminology of degrees is common in the pharmacological treatises, for example, (K) 11.561.3ff., 571.9ff., 15ff., 739.12ff., 786.11ff., 12.104.18ff., 126.9ff., 16f., 129.15ff., 132.3ff. See especially Harig 1974.

[48] Galen refers repeatedly to the ambiguities of hot, cold, wet, and dry (for example, [K] 1.476.8ff.; *Mixt.* 1.6 [H] 19.10ff., [K] 1.538.11ff.), to the difficulties of determining these qualities whether by touch or "by reason" and of discriminating between what merely appears hot, for instance, and what is really hot, whether potentially or in actuality (for example, [K] 1.381.12ff.; *Mixt.* 1.9 [H] 32.5ff., [K] 1.559.10ff., [H] 33.21ff., [K] 1.562.4ff.; 2.2 [H] 51.18ff., [K] 1.590.9ff., [H] 53.14ff., [K] 1.593.7ff.; 2.3 [H] 56.12ff., [K] 1.598.7ff.), and to the lack of any means of measuring them or of determiningthem precisely (for example, *Mixt.* 2.4 [H] 62.25ff.,

[K] 1.608.13ff., [H] 63.12ff., [K] 1.609.9ff., 10.183.3f., 650.14ff., 11.285.12ff., 544.8ff., 552.13ff., 555.17ff.). although he sometimes writes as if it were possible to attach simple numerical values to deviations from the norm and to the corrections to be made to restore it (for example, [K] 1.383.14ff.). For another ancient theorist who attempted to attach numerical values to qualitative differences, see Philoponus *In Aristotelis libros De generatione et corruptione commentaria* , 170.13ff.; cf. 148.26ff.

Aristotle's own view of the explananda, and his explanations, are both resolutely qualitative. The problems that are to be resolved**[49]** concern the qualities of perceptible substances—such properties as whether they are or are not capable of solidification, or of being melted, or of being broken—and he is satisfied with an account of compounds that specifies merely which of the simple bodies *predominates* in them.**[50]** He does not attempt to state the precise proportions of the elements in various compounds, despite the fact that in an earlier four-element theory, that of Empedocles, some admittedly hesitant steps were taken in that direction.**[51]** The fourth book of the *Meteorologica* refers often enough not only to a wide variety of compounds but also to such phenomena as evaporation and combustion.**[52]** Yet there is not a *single*

[49] See, for example, *GC* 2.2.329b7ff., *Mete.* 4.8.384b24ff., 385a10ff. While the authenticity of the fourth book of the *Meteorologica* has often been doubted (see, most recently, Furley 1983a, Strohm 1983), it may still be taken to reflect Aristotle's views on the questions that concern our discussion here.

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exact measurement in the whole book.**[53]** Here, in weighing and measuring compounds and their ingredients, there is no *technical* obstacle of the kind presented by the lack of exact time-keeping devices (and we shall be returning to consider examples where weighing is used).**[54]** But the overriding consideration favouring qualitative theories over quantitative ones is clear: the explananda themselves are conceived in qualitative terms, and any reduction to the quantitative looked vulnerable to the objection that that involved a category mistake.

Evidence for Measurement

Alongside the fields we have taken so far, which tend to support Koyré's

judgement, there are others that show that it must be substantially qualified. We may review very briefly some examples from four inquiries in turn, geophysics, astronomy, harmonics, and optics, before pursuing the issue further afield.

[54] See below at nn. 116ff.

Geophysics

First, there is a famous case from geophysics: the estimation of the size of the earth.**[55]** Our first evidence here is, once again, in Aristotle. In the course of his discussion of the shape and size of the earth in *De caelo* 2.14, he first demonstrates its sphericity and then cites a number of considerations to show that it is "of no great size," in comparison that is, with the sphere of the fixed stars.**[56]** First, he notes that observations of the stars show this: "For a small change of position on our part southwards or northwards manifestly alters the circle of the horizon. . . . Certain stars are seen in Egypt and around Cyprus which are invisible in lands towards the north, and stars that are continuously visible in northern countries set in those regions."**[57]** He ends the chapter by remarking that "those mathematicians who try to calculate the size of the circumference [of the earth] say that it is 400,000 stades."**[58]**

Aristotle does not record the method that the mathematicians in question used. However, other sources report how two later investigators proceeded.**[59]** In the third century Eratosthenes is said to have

[55] The topic has received much scholarly attention. Following such earlier discussions as Nissen 1903, Berger 1903, Drabkin 1942–43, A. Diller 1949, Dicks 1960, the debate has been reopened recently by Taisbak 1973–74; I. Fischer 1975; Neugebauer 1975, vol. 2 pp. 652ff.; Newton 1980a; Rawlins 1982a and 1982b.

[56] *Cael.* 297b30ff. Elsewhere Aristotle puts it that the earth is small by comparison with the size of some of the heavenly bodies themselves: *Mete.* 1.3.339b7ff., and cf. the ambiguous *Cael.* 298a19f., though that is not shown directly by the argument from the changes in visibility of the stars at different latitudes.

[58] *Cael.* 298a15–17.

[59] Our fullest source is Cleomedes *De motu circulari corporum caelestium*

1.10.90.20ff., but there are further references to the question, not always consistent with Cleomedes' reports, in Pliny, e.g., *HN* 2.247f. and especially Strabo, e.g., 2.5.7.

based his calculation on observations of the shadow cast by a gnomon**[60]** at noon on the day of the summer solstice at two points on the earth's circumference, namely, Alexandria and Syene, which he assumed to be on the same meridian. At Syene there was said to be no shadow,**[61]** while at Alexandria there was one of a fiftieth of a circle, i.e., seven and one-fifth degrees. Taking the distance between the two places to be 5,000 stades,**[62]** Eratosthenes arrived by simple geometry at a figure of 250,000 stades for the circumference of the earth.**[63]** Then in the first century B.C. , Posidonius is reported to have suggested a method based on comparing observations of the star Canopus above the horizon at Rhodes and at Alexandria. Taking these two locations to be 5,000 stades apart on the same meridian**[64]** and the difference in altitude of Canopus to be "a

[61] It was, however, recognised that the lack of a noon shadow at the solstice applied over a distance of 300 stades (Cleomedes 98.4f.), a point that I. Fischer 1975, p. 154, takes to be tantamount to an uncertainty statement about whether Syene lies precisely on the summer tropic: contrast Newton 1980a, p. 383, for whom this is a case of the transformation of a vague observation into a precise statement.

[63] The figure of 252,000 stades ascribed to Eratosthenes in other sources, e.g., Pliny *HN* 2.247, Strabo 2.5.7, is generally interpreted as an adjustment motivated by the wish to give a round number for each sixtieth division of the circumference of the circle (see Dicks 1960, p. 146; cf. Heath 1913, p. 339); it also yields a round number for the value of a degree (700 stades), but whether Eratosthenes already used the division of the circle into 360°—as Hipparchus later did—is not certain; see below, n. 76.

[64] Again Cleomedes does not say how this figure was obtained. According to Strabo 2.5.24, Eratosthenes distrusted the estimates given for this distance (5,000 or 4,000 stades) on the basis of reports of sailing times and got a figure of 3,750 stades from sundial observations (which, if true, would involve pre-supposing the value of the circumference of the earth and reversing the procedure used in the Syene-Alexandria case): cf. Pliny *HN* 5.132; Neugebauer 1975, vol. 2, p. 653.

quarter of a sign" (of the zodiac, i.e., seven and one-half degrees), he obtained a figure of 240,000 stades for the circumference.**[65]**

Apart from other difficulties relating to the interpretation of these reports, the accuracy of the various recorded estimates of the size of the earth has been the subject of a protracted debate. Yet this has inevitably been quite inconclusive, among other reasons because, although our sources give the figures in stades, we have no certain indication of which of the several different stades used in antiquity is in question on each occasion.**[66]** The very fact that the stade was not standardised is, of course, significant. Nor is it clear that later estimates always represent an improvement in accuracy over earlier ones, despite the assumption of steady progress that has often been made, on this and other topics, by modern commentators.

The methods used by Eratosthenes and Posidonius are certainly sound enough in principle. But in practice inaccuracies could and did

[65] Cleomedes, 94.22, adds the rider that if the distance between Rhodes and Alexandria is not 5,000 stades, the figure for the circumference will be different but in the same ratio to that distance (see Taisbak 1973–74, who stresses the hypothetical nature of the argument and suggests that Posidonius was more concerned to describe a method than to reach a result; cf. I. Fischer 1975, pp. 161f.). Again, Strabo 2.2.2 ascribes a different figure for the circumference to Posidonius, namely, 180,000 stades, which some have taken as equivalent to 240,000 stades, using a different value for the stade (see below, n. 66), while others have seen it as a revised figure (Heath 1913, pp. 345f.), though Taisbak has recently argued that Strabo's reports are internally inconsistent. Ptolemy in turn adopted 180,000 stades for the circumference and at *Geographia* 7.5.12 claimed that this is based on "the more accurate measurements," but, once again, the value of the stade may not have been the same for Posidonius as for Ptolemy.

[66] Following Hultsch 1882, pp. 42ff., Lehmann-Haupt 1929 distinguishes seven different values of the stade; cf. also Dicks 1960, pp. 42ff. At *HN* 2.247 Pliny converts Eratosthenes' stades at eight to the Roman mile, though his further report, at 12.53, that Eratosthenes took the schoenus to be forty stades has been taken to suggest a figure of ten stades to the mile, assuming the schoenus is equivalent to the parasang and so to four miles. Pliny himself, however, while commenting on the different values given to the schoenus, translates forty stades as five Roman miles.

arise at three points especially: (1) in the measurement of the angles of the sun's shadow or of the height of a star above the horizon**[67]** (in the latter case refraction would be a complicating factor); (2) in the calculation of the distances between the locations from which the observations were

made;**[68]** and (3) in the assumption that these locations are exactly on the same meridian**[69]** —although in some instances inaccuracy in one of these items acted to cancel out inaccuracy in another. Nevertheless, for our purposes the most important point is the simple one that already by Aristotle's time attempts to estimate the circumference of the earth had begun. In this context, at least, it appears that a definite quantitative result was sought, not, obviously, solely by direct measurements, but by calculation based on such measurements.

Astronomy

Astronomy offers a far richer range of examples. Koyré himself was prepared to grant that, exceptionally, Greek celestial physics was exact, but the question we must press here is whether the explanation of this

[67] Thus Posidonius' figure for the difference in altitude of Canopus (7 1/2 degrees) contrasts with an actual one of approximately 5 degrees.

[68] The problem of determining distances over land and sea continued to exercise later writers. Hero, for instance, who gives a detailed account of the construction and use of the dioptra—the chief surveying instrument used in triangulation—also describes a hodometer, a device for measuring distances on an overland journey by the automatic counting of the revolutions of a carriage wheel, thereby avoiding, as he says, the "laborious and slow" method using chains or cords (*Dioptra* 34.292.16ff.; cf. Vitruvius 10.9.1–4 with suggested adaptation for use at sea 10.9.5ff.: on the feasibility of such devices see most recently Sleeswyk 1979). The next chapter in the *Dioptra* tackles the problem of estimating greater distances, including across water, for example from Alexandria to Rome, where Hero suggests a method based on observations of a lunar eclipse (*Dioptra* 35.302.3ff.); see Neugebauer 1975, vol. 2, pp. 845ff. Ptolemy, in turn, discusses the difficulties in his *Geographia* (1.3–4) and expresses greater confidence in astronomically based calculations than in dead reckoning. Yet the former depended on accurate time-keeping, the difficulties of which we have already noted.

[69] Thus Syene is in fact some three degrees east of Alexandria, and Rhodes some one and a half degrees west of it.

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exception is the one that Koyré tended to adduce, namely, the metaphysical gulf between the superlunary and the sublunary world.**[70]**

First, it is as well to stress the hesitancy of the first steps the Greeks took in observational astronomy.**[71]** Although attempts to determine the lengths

of the solar year and the four seasons go back to the late fifth century B.C. (motivated in part, probably, by concern with calendaric problems), the number of actual observations carried out was not necessarily very great.**[72]** Even Eudoxus in the fourth century may have undertaken only limited precise observational work. One of the handicaps, at this stage, was the lack of a simple coordinate system and of the division of the globe into 360 degrees, and such evidence as we have from the fragments of Eudoxus' *Phaenomena* suggests that he identified and located individual stars quite imprecisely. Thus "beneath the tail of the Little Bear lie the feet of Cepheus making an equilateral triangle with the tip of the tail";**[73]** or, again, "over Perseus and Cassiopeia lies at no great distance the head of the Great Bear."**[74]**

By Hipparchus' time, in the second century B.C. , however, the situation had changed appreciably. First, there is firmer evidence for Greek use of Babylonian observational data, and, secondly, we have more specific information for sustained observational work carried out by the Greeks themselves, first by Timocharis and Aristyllus in the late third century,**[75]** and then by Hipparchus himself,**[76]** even though for much of

[70] See Koyré 1948/1961, pp. 312f., 1968, p. 38; cf. Sambursky 1956b, pp. 47f., and 1965.

[71] There is a brief discussion of the development of observation in early Greek astronomy in G. E. R. Lloyd 1979, pp. 169ff., and of the topic of observational error in astronomy and elsewhere in G. E. R. Lloyd 1982.

[72] See especially Aaboe and Price 1964.

[73] See Hipparchus *In Arati phaenomena (In Arat.)* 1.2.11.14.13ff., and cf. 5.19.52.1ff.

[74] Hipparchus *In Arat.* 1.6.2.54.23ff., and cf. the similar text quoted by Hipparchus from Eudoxus' *Enoptron* (Mirror) at *In Arat.* 1.6.2.56.2ff.

[75] See Ptolemy *Syntaxis* 7.1–3 especially, (H) 2.3.2ff., 12.24ff., 17.15ff., 21.16ff. The datable observations assigned to Timocharis, ranging from 295 B.C. to 272 B.C. , are set out by Pedersen 1974, appendix A, pp. 410f.

[76] The datable observations ascribed by Ptolemy to Hipparchus, ranging from 162 B.C. to 127 B.C. , are set out by Pedersen 1974, appendix A, pp. 413ff.See more generally Neugebauer 1975, vol. 1, pp. 274ff., who remarks, p. 277, that in Hipparchus' time a definite system of spherical coordinates for stellar positions did not yet exist; on the question of whether it was Hipparchus or Eratosthenes who first introduced the division of the circle into 360 degrees, see Neugebauer 1975, vol. 1, p. 305 n. 27, vol. 2, p. 590.

our evidence we continue to have to rely on such sources as Ptolemy, writing much later, in the second century A.D. Ptolemy himself not only reports his predecessors' and contemporaries' observations on many occasions but also provides the first extant comprehensive star catalogue. This is particularly valuable evidence, as the observations it is based on are not subject to interference from planetary models.**[77]** Books 7 and 8 of the *Syntaxis* give the longitudes and latitudes of over 1,000 stars in degrees and fractions of a degree, using seven simple fractions corresponding to 10', 15', 20', 30', 40', 45', and 50'.**[78]** Ptolemy tells us that he used the armillary astrolabe for these and other observations, often providing a certain amount of circumstantial detail on this.

Now, whether Ptolemy actually carried out the careful observations he says he made has become, once again, in recent years, the subject of heated controversy;**[79]** and the suggestion has been revived that his star catalogue in particular was plagiarised from Hipparchus.**[80]** The view I have argued for elsewhere is that this is an oversimplification, to say the least. Though he has taken Hipparchus' figures as his starting-point**[81]** (*not* to have done so would have been foolish), he has added

[77] The theory of the sun is, however, implicated when star positions are determined with reference to it or to the moon.

[78] Estimates are also given of the stars' magnitudes, though these are, of course, not based on measurement.

[79] See Newton 1973, 1974a, 1974b, 1977, 1980b, Hartner 1977, 1980, Moesgaard 1980b, Gingerich 1980, 1981. References to earlier literature will be found in G. E. R. Lloyd 1979, p. 184 n. 308.

[80] The idea that Ptolemy plagiarised an earlier Greek astronomer, Menelaus, was already suggested by Arabic astronomers: see Björnbo 1901; Dreyer 1916–17, pp. 533ff.; Vogt 1925, pp. 37f.

[81] Perhaps Newton's most telling argument is based on an analysis of the pattern of error in Ptolemy's catalogue: Newton 1977, pp. 237ff., 1979, pp. 383ff.

stars that were not included by Hipparchus, and where comparisons are possible, these suggest that he has done more than just take over Hipparchus' results and adjust these for precession.**[82]** However, the ramifications of this controversy need not detain us further at this point, for the simple reason that whoever was chiefly responsible, whether Hipparchus or Ptolemy, the catalogue as we have it is excellent evidence of sustained observations. It reveals both the degree of *precision* aimed at (of the order of 10') and the *accuracy* obtained (the mean error in longitude is of the order of a degree; in latitude, of half a degree).**[83]**

When we turn to the observations carried out in connection with the determinations of the parameters of astronomical models, the picture is complicated, in Ptolemy's case especially, by that controversy over the issue of the match—or mismatch—between his protestations of a concern for accuracy and his actual practice. Yet, to begin with the protestations, the evidence that both Ptolemy and, before him, Hipparchus were at pains to draw attention to the problems posed by the reliability of the data they had to work with is impressive. Ptolemy often expresses his qualms about the accuracy of some of the observations conducted by earlier astronomers, criticising their rough-and-ready character, and he indicates that Hipparchus already had similar doubts or reservations.**[84]** They were also alert to the differences in reliability of different kinds of data. Those derived from eclipses or occultations were recognised as more trustworthy than those involving estimates of wide angular distances or of absolute positions. Thus, Hipparchus used lunar eclipse data for his theory of the moon, even though these presupposed, of course, his model for the sun.**[85]**

Furthermore, both Hipparchus and Ptolemy drew attention to particular sources of inaccuracy in both naked eye and instrumentally

[82] G. E. R. Lloyd 1979, p. 184; cf. Gingerich 1981, pp. 42f.

[83] Cf. Toomer 1984, p. 328 n. 51.

[84] See *Syntaxis* 3.1 (H) 1.203.7ff., 14f., 205.15ff.; 7.1 (H) 2.2.22ff., 3.4f.; 9.2 (H) 2.209.5ff.

[85] See *Syntaxis* 4.5 (H) 1.294.21ff.; cf. 4.1 (H) 1.265.18ff.

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aided observations.**[86]** Ptolemy refers to distortions due to atmospheric conditions or to the object being close to the horizon; in his *Optics* (though not generally in the *Syntaxis*) atmospheric refraction is discussed.**[87]** The *Syntaxis* includes descriptions of the main astronomical instruments used,

sometimes, though not always, with specifications concerning their size and construction,**[88]** and it issues warnings about particular sources of inaccuracy in their use. In one notable passage, where again he is following Hipparchus' lead,**[89]** Ptolemy writes of the errors arising from the faulty positioning or calibration of instruments. Referring to the use of equatorial armillaries, he notes that a deviation of a mere six minutes of arc from the equatorial plane in the setting of the instruments generates an error of six hours in determining the time of the equinox,**[90]** and of the bronze rings in the Palaestra at Alexandria he remarks: "For so great is the distortion in their position, and espe-

[87] Ptolemy *Optics* 5.23ff. (237.20ff. Lejeune); cf. also Cleomedes 2.6.222.28ff., 224.11ff. Distortions due to the object being near the horizon are referred to in Ptolemy's *Syntaxis* at, for example, 1.3 (H) 1.11.20ff., 13.3ff.; 9.2 (H) 2.209.16f., 210.3ff. (where it has been thought that refraction is possibly in mind: see Toomer 1984, p. 421 n. 8).

[88] See *Syntaxis* 1.12 (H) 1.64.12ff., 66.5ff.; 5.1 (H) 1.351.5ff.; 5.12 (H) 1.403.9ff.; 5.14 (H) 1.417.1ff. There are useful brief surveys of ancient astronomical instruments in Dicks 1953–54 and Price 1957.

[90] *Syntaxis* 3.1 (H) 1.196.21ff.

cially in that of the bigger and older one, when we make our observations, that sometimes their concave surfaces twice suffer a shift in lighting in the same equinoxes."**[91]**

To be sure, these expressions of a concern for accuracy have to be judged against actual performance. So far as Ptolemy goes, certain aspects of his procedures are not disputed and are indeed transparent enough. He repeatedly has recourse, throughout his calculations, to approximations and rounding procedures, some but not all of which he explicitly signals as such. Moreover, as the most recent detailed recalculation of his results goes to confirm,**[92]** quite a number of those approximations are biased towards establishing a preconceived value, often one he believes to have the authority of Hipparchus in particular or of tradition in general behind it. Sometimes he may well have worked back from such a result, not merely in that it influenced the approximations he introduced but also in his selection of the observations he presented.**[93]**

Equally, though, there are occasions when Ptolemy records data that do not *simply* confirm his conclusions—the very data on which the charge of fabrication has sometimes then been based.**[94]** Furthermore, in two cases, his theories of the moon and of Mercury, he made substantial modifications

in his usual epicycle-eccentric model, introducing in both instances an extra circle in addition to the epicycle and the deferent.**[95]** Here the very complexities he thought necessary appear

[92] See Toomer 1984.

[93] See, for example, Czwalina 1959; cf. Newton 1977, pp. 266 and 307; Gingerich 1980, pp. 260ff.

[94] As, for example, in the case of the two sets of data presented in Ptolemy's discussion of the value of precession: *Syntaxis* 7.2–3 (H) 2.19.16ff., 25.13ff.

[95] For an account of the Mercury and moon models, see, for example, Pedersen 1974, pp. 159ff., especially pp. 192ff. and pp. 309ff.; Neugebauer 1975, vol. 1, pp. 68ff., 84ff., and 158ff.

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to be quite gratuitous unless they are a response to what *he* perceived to be mismatches between the simple model and *some* empirical data, however and by whomsoever these were obtained.**[96]** Many of his procedures would be considered sharp practice, as well as slapdash, today—in some cases also, maybe, in his own day. At the same time, there are many contexts in which his practice can be taken to bear out, at least to some extent, his expressed concern over securing a comprehensive and reliable data base.

However hesitant its beginnings, Greek astronomy eventually achieved outstanding successes in developing detailed, quantitative models to account for complex natural phenomena. The mathematical models themselves were rigorous exercises in deductive geometry. But they were evaluated not just as geometry but on how well they matched the data—an essential point we shall return to in Chapter 6.**[97]** Greek astronomers were certainly neither as active nor as systematic as they might have been in confronting—or in recording the confrontations between—predicted theoretical positions and actual sightings. Yet from Hipparchus onwards, and I should say including Ptolemy, the quality of the data obtainable was a major preoccupation, not just in principle but also in practice. The rigour and exactness of the inquiry were its pride. But the point was not—or was not so much—that astronomy deals with the unchanging heavens, as, more simply, that it is based on mathematics.**[98]** In particular, the realisation that the exact-

[96] Cf., e.g., Gingerich 1980, pp. 261f.: "Ptolemy must surely have put credence in some specific observations here, or he would not have ended up with such an unnecessarily complicated mechanism for Mercury."

[97] See below, Chap. 6, pp. 304ff., 312ff.

ness and reliability of the data *vary* in different contexts is important, since it shows that there is nothing *automatic* about the accuracy of the data and that the *degree* of accuracy was a matter that had to be evaluated in the given circumstances of each part of the inquiry.

Harmonics

Two other areas of investigation, neither of which is tied to superlunary phenomena and both of which are regularly hailed as mathematical, will enable us to test the points I have just made. In harmonics there is a long-drawn-out dispute over the status of the perceptible phenomena, where the positions adopted range from an extreme empiricism all the way to the bid to reduce harmonics to pure number theory.**[99]** How far a particular investigator was committed to a search for exact quantitative data would depend on his position in that overall epistemological controversy. But, as is well known, Plato already knew a tradition in which the *measurement* of the phenomena was fundamental. In the seventh book of the *Republic* , 530dff., Socrates first agrees with a view he ascribes to the Pythagoreans, that harmonics and astronomy are sister sciences, but then he goes on to criticise as "useless labour" the business of *measuring* (

ἀναμετροῦντες

, 531a2) audible sounds and concords against one another. This contrasts, rather—at least at first sight—with the approval of measurement to deal with certain optical effects expressed later, in *Republic* book 10.**[100]**

[99] We shall consider this below, Chap. 6 at nn. 41ff.

Moreover, elsewhere Plato provides some of our best early evidence for a recognition of the point that the exactness of sciences varies with their use of measurement, when, in the *Philebus* , branches of knowledge are stratified according to this criterion.**[101]** But in *Republic* book 7, after Socrates' critical remark, Glaucon too speaks of those who "lay their ears alongside" the strings, "as if trying to catch a voice from next door; and some state that they can hear another note in between and that this is the

smallest interval which is to be used as a *unit of measurement* , while others contest that the sounds are the same, both parties preferring their ears to their minds."**[102]** Socrates distinguishes

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these ultra-empiricists from the Pythagoreans,**[103]** but the latter too are criticised for "*looking for numbers* in these heard concords and not ascending to problems."**[104]**

Plato's testimony here is all the more impressive in that he is, at this point, a hostile witness. He disapproves of the methods he describes, at least for his present concerns, and insists that it is only the completely abstract study (the consideration of which numbers are concordant with one another and which not)**[105]** that is to be included in the educational programme of the Guardians. But *measurement* is an integral part of the procedures he criticises, indeed, *both* those of the ultra-empiricists who were engaged in an attempt to establish an audible minimum which could serve as a unit of measurement,**[106]***and* those of the Pythagoreans in their search for numbers in heard concords. In the latter case we have other evidence concerning Pythagorean investigations—for example, on the monochord**[107]** —and it is clear that they had a particular motive for this study, namely, the bid to illustrate and

[103] *R.* 531b7; cf. 530d6ff.

[105] See *R.* 531c3–4.

[106] *R.* 531b2ff. suggests the use of at least two strings, tuned initially within a small interval of one another, one or both of which are then tightened or slackened to try to detect the point at which the audible difference disappears: I am grateful to Dr. Andrew Barker for clarification of the interpretation of this passage.

[107] See especially Burkert 1972, chap. 5, especially pp. 374ff., and cf. further below, Chap. 6 at nn. 37ff. It is, however, noteworthy that in one of the most substantial pieces of direct evidence we have concerning one of the more prominent Pythagorean theorists, namely, Archytas, various pieces of what purport to be empirical evidence are adduced to support a general conclusion concerning the correlation between pitch and the speed of a note, but no precise measurements are attempted: see Archytas fr. 1, Porphyry *In Ptolemaei Harmonica* (*In Harm.*) 56.5–57.27 Düring; cf. Ptolemy *Harmonica* (*Harm.*) 1.13.30.9ff. Düring; Porph. *In Harm.* 107.15ff. D.; Boethius *De institutione musica* (*Mus.*) 3.11.285.9ff.; Theon 61.11ff.

support the doctrine that "all things are numbers."**[108]** Yet the contrast between the Pythagoreans and the ultra-empiricists shows that Plato had others in mind as well. Here, then, in an admittedly simple case, we can say that empirical inquiries involving measurement were undertaken before Plato—and we can follow their fortunes (not always auspicious fortunes, to be sure) in a long line of writers on harmonics from Aristoxenus down to Ptolemy, Porphyry, and beyond, though—to repeat—the importance attached to such investigations and the status accorded to the information obtained vary from one writer to another.**[109]** Harmonics is, however, certainly one of the first examples of the successful quantitative explanation of certain qualitative phenomena.

Optics

The evidence we have for the early stages of the development of optics relates mainly to certain purely geometrical aspects of the study.**[110]** Euclid's own optical treatise first sets out certain assumptions about

[108] We shall be returning to discuss this doctrine below, pp. 275ff.

[110] Aristotle already includes optics, along with harmonics and astronomy, among the "more physical of the mathematical inquiries" (*Ph.* 194a7f.), but the direct evidence for pre-Euclidean work is very limited: cf. Lejeune 1948, Mugler 1957, 1958.

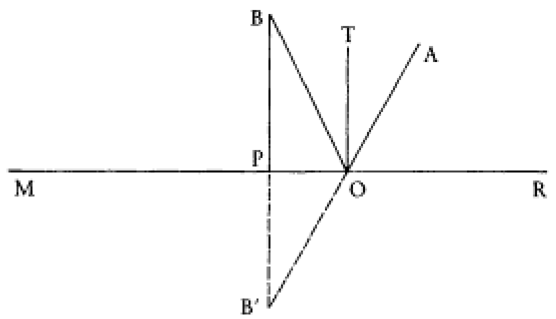

Figure 1
After Cohen and Drabkin, edd., 1958, p. 269 n. 1.

light rays and then proceeds deductively, *more geometrico* .**[111]** However, empirical tests confirming the laws of reflection are described, for instance, in Ptolemy's *Optics* , although we cannot pinpoint the date of their discovery.

He first sets out the three elementary laws (3.3.88.9ff. Lejeune): (1) objects that are seen in mirrors are seen in the direction of the visual ray that falls on them when reflected by the mirror; (2) things that are seen in mirrors are seen on the perpendicular that falls from the object to the surface of the mirror and is produced; and (3) the position of the reflected ray, from the eye to the mirror and from the mirror to the object, is such that each of its two parts contains the point of reflection and makes equal angles with the perpendicular to the mirror at that point. With reference to Figure 1, where MR is the mirror, A the eye, B the object, B' the image, O the point at which the visual ray

[111] As with the *Elements* , the textual tradition of Euclid's *Optics* has been subject to much reworking. One of the two extant versions is the result of editing by Theon of Alexandria in the late fourth century A.D. , and we cannot assume that the other has escaped similar revision and correction. Both versions are, however, strongly characterised by the deductive geometrical treatment of the problems, even though, as Suppes 1981 has recently stressed, the axiomatisation is, by modern standards, quite incomplete: cf. also Lear 1982, p. 189 n. 36.

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strikes the mirror, and TO and BP perpendiculars to the mirror, these three laws state: (1)B' lies on AO produced; (2) B'lies on BP produced; (3) *Ð* TOA = *Ð* TOB. He then provides experimental confirmation of these (3.4–13.89.4ff. Lejeune).**[112]**

For our purposes the evidence for investigations of refraction is particularly important since, to judge again from Ptolemy, they included not just general discussions of the phenomenon but also measurements of its amount for different pairs of media carried out with apparatus that he is at pains to describe (*Optics* 5.8 [227.5ff. Lejeune]). The tables in *Optics* 5.11, 18, and 21 (229.1ff., 233.10ff., 236.4ff. L) setting out the amount of refraction for angles of incidence at 10° intervals from 10° to 80° first for air to water, then for air to glass, and then for water to glass, are remarkable from several points of view. They provide one of the clearest cases of an ancient scientist doctoring his results. Ptolemy has evidently adjusted these to fit his general law, even though that law itself is not stated. This takes the form $r = ai - bi^2$, where r is the angle of refraction, i the angle of incidence (the incident ray being that from the eye to the refracting surface), and a and b constants for the media concerned.**[113]** Nevertheless, the complexities of that general law are *quite unmotivated* unless Ptolemy has made *some* (and we may believe quite extensive)**[114]** empirical investigations involv-

[112] For discussion of these laws and of their pre-Ptolemaic background, see Boyer 1945–46; Lejeune 1946, 1957, pp. 47ff.

[113] The equation stated is one formulated by Govi 1885, p. xxxiii, but as Lejeune 1940–46, pp. 97ff., noted, it does not depend on the use of an algebraic expression, for Ptolemy could easily have set out the relationship in words; moreover, the use of first and second differences had been standard in other contexts, such as astronomy, since Babylonian times. A. M. Smith 1982, p. 237, has, however, recently argued against the view that Ptolemy had any such general theory, and indeed, that he was unable to find the law of second differences for both mathematical and methodological reasons. Yet in view of the fact that every one of the results in all three tables tallies exactly with this law and that they do so even where there are notable discrepancies between them and what the application of the sine law would give, this must be thought to make this highly unlikely.

[114] See Lejeune 1940–46, p. 94. A. M. Smith 1982, p. 234, also assumes that Ptolemy had observational data with a high degree of accuracy.

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ing the measurement of angles of incidence and of refraction for these media—even if those investigations did not yield quite the results that were claimed.**[115]**

Weighing

So far I have concentrated exclusively on the exact sciences. But one simple measuring technique used in a wide variety of contexts was *weighing* ,**[116]** and this will now take us further afield, including into what we call the life sciences. Heavy and light were often cited as, or among, the differentiae of natural substances in both physics and physiology, but we must be careful, since they are sometimes understood in purely qualitative terms, on a par with wet and dry, or sweet, salty, and bitter.**[117]** Thus when in certain contexts in his mineralogical work *On Stones* Theophrastus differentiates varieties of pumice or of

[115] Lejeune 1940–46, p. 97, suggests that the second difference was applied to the middle range in the tables and that Ptolemy extrapolated from these to the (generally less accurate) results claimed for the extreme cases of angles of incidence for 10° and (especially) 80°. On the other hand, provided we assume, as in the cases Ptolemy discusses, that the incident ray from the eye passes from the less dense to the denser medium, the generalisations he sets out at 5.34.245.1ff. L, are unobjectionable, namely, that where i' is greater than i , (1) $i' : i > r' : r$, (2) $i' : r' > i : r$, and (3) $(i' - i) : i > (r' - r) : r$.

[116] Written evidence for standardised weights in Greece goes back to the Mycenaean period: see Chadwick 1973, pp. 54–58. Moreover, the archaeological record provides evidence for the standardisation of weights in the ancient Near East and the Indus valley at a much earlier date: see Hemmy 1931; F. G. Skinner 1954, pp. 779ff.

[117] Thus at *GC* 329b18ff. (cf., e.g., *GC* 326a7f., 329a10ff.), Aristotle lists heavy and light along with hot and cold, dry and wet, hard and soft, viscous and brittle, rough and smooth, dense and rare, among the tangible contrarieties. Moreover, when taken as definable in terms of a natural tendency to move in a certain direction (up/down), heavy and light run counter to the stipulation, in the *Categories* 5b11ff., that quantities have no contraries. However, at *Metaph.* 1052b18–31 Aristotle includes weight with length, breadth, depth, and speed among examples of what can be measured, where it meets the criterion that there must be units or standards of measurement by which weight can be determined.

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metal-bearing ore by "heaviness,"**[118]** no actual quantities are mentioned. "Pumices," he says, "differ from one another in colour, density, and heaviness. They differ in colour inasmuch as the pumice from the Sicilian lava-flow is black, while in density and heaviness it is quite like a millstone. For pumice of this kind does indeed exist, heavy and dense and more valuable in use than the other kind. This pumice from the lava-flow is a better abrasive than the kind which is light [in weight] and white in colour, although that which comes actually from the sea is the best abrasive of all."**[119]**

Elsewhere, however, direct reference is made to *weighing* to distinguish heavier and lighter kinds of the same substance. The Hippocratic treatise *On Airs Waters Places* is much preoccupied with the differences in the waters that occur in different places, distinguishing those that are "hot" and "cold," "hard" and "soft," stagnant and free-running, turbid and pure and bright, as well as—frequently—those that are "heavy" and those that are "light."**[120]** The opening chapter

[119] Theophrastus *Lap.* 22. Cf. *Lap.* 39: "There are also many kinds of stones extracted from mines. Of these some contain gold and silver, though only the silver is clearly perceptible: they are rather heavy and strong-smelling. . . . There is also another stone like charcoal in colour, but heavy." At *Lap.* 46 the quantities of metals in gold alloys are said to be determinable by the use of the touchstone.

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suggests that here we are dealing not just with vague general impressions, but with something measurable, for there we are told that waters "vary both in taste and 'on the balance.'"**[121]**

Measurement is also clearly involved in Archimedes' famous hydrostatical investigations. The story of how he detected the adulteration of

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a gold crown by observing that it displaced more water than the equivalent weight of pure gold may well be inaccurate in the form in which we have it from Vitruvius.**[122]** But the extant treatise *On Floating Bodies* shows that he had a clear working conception of—even if he does not explicitly formulate—what we call specific gravity.**[123]** In book 1, chapters 3ff., he distinguishes between solids that are "equal in weight" (

ἰσοβαρέοντα

) with a given fluid, those that are "heavier" and those that are "lighter" than it, where he clearly has in mind not absolute weight but weight in relation to a given volume,**[124]** and in chapter 7 he enunciates the principle since named after him: "solids heavier than the fluid will, if placed in the fluid, be carried down to the bottom of the fluid, and they will be lighter in the fluid by the weight of the amount of fluid that has the same volume as the solid."**[125]**

Further evidence from the medical writers shows that they referred readily enough to weighing and measuring in particular contexts. For instance, in their pharmacology, the proportions of the ingredients in compound drugs, and the dose to be used, are often—though certainly far from invariably—specified by weight or otherwise by exact quantity, that is, by dry or liquid measure.**[126]** Thus *On the Diseases of*

[122] Vitruvius 9 praef. 9ff.

[123] The Arabic writer Al Khazini ascribes to Archimedes a device that could be used to determine relative specific gravities of different metals when weighed in water, in the *Book of the Balance of Wisdom* 4.1, on which see, for example, Knorr 1982b.

[124] *De corporibus fluitantibus* (*Fluit.*) 1.3ff. (HS) 2.320.32ff.

[126] Already much earlier in Egyptian pharmacology, quantities are sometimes specified (as, for example, in para. 2 of the Papyrus Ebers: "to expel diseases in the belly: Another [remedy] for the belly, when it is ill: cumin 1/2 ro,goosefat 4 ro, milk 20 ro, are boiled, strained and taken. Another: figs 4 ro, sebesten 4 ro, sweet beer 20 ro, likewise" [Ebbell 1937, p. 30]), though this is not invariably the case (cf. para. 3 of the Papyrus Ebers: "another: wine, honey, colocynth, are strained and taken in one day" [Ebbell 1937, p. 31]). F. L. Griffith 1898, pp. 5ff., commenting on the prescriptions in the Petrie papyri, noted that the "quantities to be used are often left to the discretion of the practitioner to determine; but where necessary the amount is specified, though in round terms, by measure and not by weight," and he went on to argue that "a great advance was made when weight was substituted for measure, as in the Greek medical works." As we shall see, however, there is still plenty of indeterminacy in Hippocratic prescriptions too, as well as in those of later periods. On the measures used in the Ebers Papyrus, see Ebers 1890; for a comparison between Greek and ancient Near Eastern pharmacological recipes, see Goltz 1974, Harig 1975, 1977, 1980, Harig and Kollesch 1977. On the possibility of the deliberate withholding of information concerning quantities for reasons of secrecy, see, for example, Goody 1977, pp. 137f.

Women book 1 gives this prescription to promote parturition: "one obol of dittany, one obol of myrrh, two obols of anis, one obol of nitre: pound these till they are smooth, pour on them a cyathus of sweet wine and two cyathi of hot water; give to the patient to drink and wash her in warm water."**[127]** Many similar examples could be given—though

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so too can others where the quantity of one or more of the ingredients, or the dose, is *not* specified exactly,**[128]** and after the Hippocratics, references to the problems of the standardisation of weights and measures and of correlating those used in different parts of the Greco-Roman world appear in the pharmacological sections of such writers as Celsus, Scribonius Largus, and Galen,**[129]** while tables of weights and measures begin to become common in specialist metrological writings.**[130]**

[129] See, for example, Celsus *Med.* 5.17.1c, *CML* 1.194.5ff.; Scribonius Largus praef. 15.5.23ff.; Galen (K) 13.435.1ff., 616.1ff., 789.2ff., 893.4ff. Cf. also Pliny *HN* 21.185 (though at 22.117–18 Pliny says that it is not possible to weigh out the powers of drugs "scruple by scruple," and at 29.24f. remarks that Mithridates' antidote that contains fifty-four ingredients no two of which have the same weight is clearly the product of ostentatious

boasting).

[130] The remains of Greek and Roman metrological writings have been collected by Hultsch 1864 and 1866. The treatise devoted to weights and mea-sures in the Galenic corpus, (K) 19.748.1–781.3, is spurious, as is some of the corresponding material in the works of Hero: see Hultsch 1882, pp. 7ff.

A twofold contrast suggests itself. On the one hand, the simpler notion, found already in Empedocles' element theory,**[131]** that a compound consists of certain *proportions* of the constituent substances may be contrasted with the more precise idea that the quantities of the constituents are to be determined *by weight* .**[132]** Yet on the other, despite the progress made towards exact quantitative specification, that progress was still very incomplete. Moreover, quantitative specification when we find it—even when all the relevant quantities are stated—was often no more than window-dressing.

In interpreting this evidence we have to bear in mind, first, that the ingredients used are not chemically pure substances, and, secondly, that ancient doctors are frequently urged to modify the drug and the dosages *in relation to particular patients* .**[133]** Thirdly, as we noted in Chapter 3, some early medical writers insist that medicine, though a genuine *techne* , art or skill, cannot be made an *exact* study,**[134]** and

[131] Empedocles frr. 96 and 98.

[132] Apart from in the pharmacological contexts we have considered, the specification of the weights and measures of ingredients is common also in the extant Greek chemical and alchemical texts. See, for example, from the Leyden Papyrus X, pagina 1a.21ff. and 25ff. (Leemans 1885, p. 205); pag. 8a.28ff. (p. 225); pag. 11a.8ff. (p. 233); 24ff. (p. 235); Halleux 1981, nos. 4, 5, 56, 81, and 83; and Berthelot and Ruelle 1888, part 1.13.10ff., 2.31.7ff., part 4.19.1ff., 2.285.6ff. Cf. also Preisendanz 1973–74, P. 12.193ff., 2 p. 71. Although the reactions of various natural substances to fire were often remarked on, for example by Aristotle (cf. above, n. 52) and by Theophrastus, especially *Lap.* 9–17, no ancient scientist thought to make systematic observations of the weights of substances before and after combustion. Vitruvius 2.5.3, however, does note that in the manufacture of quicklime "about a third" of the weight of the stone is lost.

[133] See, for example, *Vict.* 1.2 (L) 6.470.7, 14ff.; *Mul.* 2.192 (L) 8.372.7ff.; cf., e.g., Pliny *HN* 25.150. Alternatively the dose is to be modified in accordance with the strength of the disease, as, e.g., at *Mul.* 1.78 (L)

8.184.17. It may also be noted that the problem of the identification of the active ingredients in compound drugs is further complicated when beliefs about their interactions, including their "sympathies" and "antipathies," have to be taken into account: cf. Pliny *HN* 22.106. Cf. Müri 1950, p. 189; Harig 1974, pp. 64ff., 83ff., 133ff., and 1980.

[134] Cf. above, Chap. 3 at nn. 89–103, on texts in *VM, Morb.* 1, *Loc.Hom.* and *Vict.* 3, especially.

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some object specifically to appeals to such a procedure as weighing. When the writer of *On Ancient Medicine* protests that exactness in the control of diet is difficult to achieve, he says that "one should aim at some measure,"**[135]** but he then goes on: "but as a measure you will find *neither number nor weight* by referring to which you will know what is exact, and no other measure than the feeling of the body."**[136]** The treatise *On Sterile Women* , too, writes that treatment should be adapted to the particular patient, having regard to her condition and strength, which are *not* a matter of *weighing* ,

τούτων γὰρ οὐδεὶς

σταθμός ἐστιν

.**[137]** The question of when it is appropriate to have recourse to weighing was, in fact, a matter of dispute, for some writers were for making medicine exact, or at least for representing it as such,**[138]** while others were suspicious of attempts to do so and critical of what I have just called window-dressing. Nevertheless, *some* reference to weighing and measuring in pharmacological contexts is common enough, even if often the concern is not so much with exact formulae as with the proportionalities between the "strength" of the drug and that of the patient.

To these pharmacological cases we can add an admittedly limited number of other examples from medical writers at different periods where quantitative reasoning is in play in various physiological or pathological contexts. In the general description of the climatic and

[137] *Steril.* 230 (L) 8.444.1f.

[138] Cf. above, Chap. 3 at nn. 26ff., on the dogmatic claims to certain knowledge in such treatises as *De arte* , and below at nn. 150ff. on Hippocratic numerology.

epidemiological conditions encountered that is set out in the Constitution in *Epidemics* book 3, it is remarked, at one point, that the urine discharged was out of proportion to the fluid drunk, though here no specific quantities are mentioned.**[139]** In one of the case-histories in *Epidemics* book 7, however, we are told that a patient discharged more than a chous**[140]** of fresh blood in his stool and then, after a short while, a further third of a chous of coagulated globlets.**[141]** Specifications of the quantities of the lochial discharge or of the menses are also sometimes given in the gynaecological and the embryological treatises—though in several cases the quantities reported appear fanciful.**[142]**

Then Erasistratus, in a remarkable experiment recorded in Anonymus Londinensis,**[143]** tried to prove that animals emit invisible effluvia, by keeping a bird in a closed vessel without food for a period and then weighing the bird and its visible excreta. Comparing this with the original weight, he found, we are told, that there had been a "great loss of weight"—another case where, in our source at any rate, an observed *difference* in weight is remarked without any *actual* weights being reported.**[144]**

[139] *Epid.* 3.10 (L) 3.90.7f., cf., e.g., *Morb.* 4.42 (L) 7.564.4ff.

[140] A *chous* is estimated as between 2.52 and 3.96 litres in OCD .

[141] *Epid.* 7.10 (L) 5.380.20ff.; cf., e.g., 7.3 (L) 5.370.23ff., 372.1ff., where the exceptional quantities of milk consumed by a particular patient are specified; *Epid.* 5.14 (L) 5.214.1ff., 5.18 (L) 5.218.10; 5.50 (L) 5.236.16.

[142] See *Mul.* 1.6 (L) 8.30.8ff.: menses of two Attic cotylae "or a little more or less," i.e., c. 0.45 litres (cf. Aristotle, who claimed generally that female humans produce more menses than any other animal, e.g., *HA* 521a26f., and estimated the discharge of a cow in heat as "about half a cotyle or a little less," *HA* 573a5ff.; and contrast Soranus *Gyn.* 1.20, *CMG* 4.14.4, who gives a maximum figure for menstruation as two cotylae but who then devotes two chapters to pointing out how the quantity and duration may vary, 1.21–22, *CMG* 4.14.6ff., 15.1ff.). *Mul.* 1.72 (L) 8.152.3ff., *Nat.Puer.* 18 (L) 7.502.3ff.: the lochial discharge is one and a half Attic cotylae "at first" "or a little more" (*Nat.Puer.* adds "or a little less"). For discussion of these figures, see Bourgey 1953, p. 178 and n. 2; R. Joly 1970, p. 62 n. 2; Lonie 1981a, pp. 190ff.

[143] Anon. Lond. 33.43ff.; see von Staden 1975, pp. 179ff., and forthcoming. Further tests involving the weighing of fresh and "high" meat, and of a bladder empty and full of air, are reported in other contexts in

Anon. Lond. at 31.10ff., 34ff. (purporting to present an Empiricist view), 32.22ff.

[144] It appears from a report in Galen *UP* 7.8 (H) 1.392.25ff., (K)3.540.8ff., that Erasistratus attempted to distinguish between different types of "air" by their "thinness" and "thickness," claiming that the air from burning coals is "thinner" than "pure" air, but Galen records no measurement in this connection.

Galen, especially, uses quantitative arguments on several occasions. In *On the Use of Parts* he remarks generally on the proportionalities between the fluids and solids taken into the body and those discharged or lost,**[145]** and elsewhere he specifies actual amounts of, for example, pus expectorated.**[146]** In *On the Natural Faculties* the difference in size between, on the one hand, the vena cava (together with the right auricle) and, on the other, the pulmonary artery is cited among the arguments to support the conclusion that some blood must pass directly from the right ventricle to the left through invisible pores in the septum, though—unlike Harvey—Galen does not attempt to measure the quantities or flow of blood exactly or even approximately.**[147]** Most notably of all, perhaps, a quantitative argument is adduced in the refutation of Lycus' view that urine is the residue from the nourishment of the kidneys.**[148]** That cannot be the case, Galen claims, if one considers the amounts discharged, which in exceptional cases may be as much as three or four choes.**[149]** If that is produced from nourishing the

[145] See *UP* 4.13 (H) 1.223.10ff., (K) 3.304.7ff. (where the quantity of drink consumed is proportional to the urine discharged), and *UP* 16.14 (H) 2.433.4ff., (K) 4.340.2ff. (where the nourishment taken in is equal to the material lost from the body).

[146] E.g., (K) 8.321.15ff. Cf. also (K) 11.227.9ff., blood expectorated up to two cotylae.

[147] *De naturalibus facultatibus* (*Nat.Fac.*) 3.15 (H) 3.252.13ff., (K) 2.208.11ff. Cf. *UP* 6.17 (H) 1.362.7ff., (K) 3.497.9ff., where Galen reverses the explanation, putting it that there is good reason for the vena cava to be larger than the pulmonary artery, since blood is taken over from the right ventricle to the left through the interventricular pores.

[148] *Nat.Fac.* 1.17 (H) 3.152.17ff., (K) 2.70.10ff., on which see Temkin 1961, and cf. Temkin 1973, pp. 153f.

[149] *Nat.Fac.* 1.17 (H) 3.153.23ff., (K) 2.72.4ff.; cf. also (H) 3.153.13ff., (K) 2.71.12ff. Altman and Dittmer 1972–74, vol. 3, p. 1496, give a normal figure, for a 70 kg body, of 1.4 litres, with upper and lower limits of 2.94 and 0.49 litres. Galen's "three choes" is clearly more than five times the normal figure and more than twice the upper limit.

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kidneys, one would expect even greater amounts of residue from the nourishment of the other principal viscera, where there is no sign of this.

Counting

Exactness in the medical writers is sometimes a matter not of weighing or measuring, but of *counting* .**[150]** Great importance is attached by many Hippocratic authors to the study of numerical relationships in connection with the determination of periodicities, notably in two types of context: (1) pregnancy and childbirth; and (2) the phases of diseases, especially their "crises," the points at which exacerbations or remissions are to be expected. In both contexts some of the ideas expressed have a solid basis. The normal time of gestation in humans is fixed to within fairly well-defined limits.**[151]** Before the advent of anti-biotics, studies were carried out that went to show that certain acute conditions such as certain pneumonias and malaria manifest quite marked periodicities.**[152]** In both fields, however, the proposals about periods and relations made in some Hippocratic texts go far beyond the range of what could be justified fairly straightforwardly by appeals to readily accessible evidence. Here the search for exactness led not to Koyré's "universe of precision" but to spurious quantification and ad hoc numerological elaboration.**[153]**

[150] The relationship between measuring and counting is discussed by Aristotle at, for example, *Metaph.* 1052b18ff., 1088a4–11, *Ph.* 220b18ff.: normally, counting is deemed a kind of measuring, but at *Metaph.* 1020a8ff. the two are contrasted where he distinguishes numbering quantities constituted by discontinuous parts and measuring magnitudes that are continua.

[151] Altman and Dittmer 1972–74, vol. 1, pp. 137f., specify a range of 253 to 303 days for humans and give corresponding figures for various other species of animals. Apart from in the medical writers, an interest in the topic is shown by Aristotle, who represents humans as exceptional among animals in the variation shown in the times of gestation of viable infants, e.g., *HA* 584a33ff., *GA* 772b7ff., and cf. *Problemata* 10.41.895a24ff.

[152] See, for example, Musser and Norris, cited by Osler 1947, pp. 49f., on pneumococcus lobar pneumonia, and Osler 1947, p. 491, on malaria.

[153] Aspects of this question have been discussed by Lichtenthaeler 1963,pp. 109ff.; R. Joly 1966, pp. 108ff.; Heinimann 1975; and Kudlien 1980, especially.

Number lore in Greek medicine must be interpreted in part against a background of Pythagorean beliefs, not just the general doctrine that "all things are numbers" but also more particular ideas concerning, for example, the importance of odd and even numbers and the correlation of that pair with other pairs of opposites. Odd is associated with right, male, and good, and even with left, female, and bad in the Table of Opposites reported by Aristotle, and we have other evidence that suggests that above/below, front/back, and other pairs were also sometimes incorporated into similar schemata.**[154]**

Yet the patterns of beliefs to which the medical theories we are interested in can be related include much besides Pythagoreanism. Many of the ideas attributed to the Pythagoreans are, in any case, widespread in popular belief. The positive and negative associations of some of the pairs of opposites included in the

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certainly antedate Pythagoras.**[155]** The classification of numbers into odd and even is general throughout Greek arithmetic. The idea that the days of the month may be good or evil can be traced back to Hesiod.**[156]** Among other aspects of number lore, the idea of the special significance of the number seven occurs in sources before Pythagoras, notably in a famous poem of Solon's, not to mention the more controversial question of possible non-Greek influences dating from earlier still.**[157]**

[155] Cf G. E. R. Lloyd 1966, part 1, especially pp. 41ff.

[156] Hesiod *Op.* 765ff., 822ff.

[157] Solon 19 Diehl, and cf. below at n. 208, on Aristotle's criticism of farfetched theories correlating sevens. On the provenance of the ideas set out in the Hippocratic treatise *Hebd.* and on the date of that work, see Mansfeld 1971 with references to earlier literature. On the question of Near Eastern parallels to, and possible influence on, Greek ideas here, see Roscher 1904, pp. 85ff., 1906, 1911, p. 10 n. 9; Götze 1923; Reitzenstein and Schaeder 1926; Kranz 1938b; Mansfeld 1971, pp. 21ff. and 65; Burkert 1972, pp. 468ff.

We must recognise at the outset, therefore, that the pattern of beliefs against which Hippocratic numerological ideas are to be judged is complex. Moreover, those ideas themselves are extraordinarily heterogeneous. We may begin with some of those connected with pregnancy and childbirth. It would, of course, be futile to attempt to determine at what stage the Greeks were aware of the approximate time of gestation of the human embryo. When we reach the classical period, the view that babies born in the seventh, ninth, or tenth month are viable, whereas those born in the eighth month generally die, is widespread.**[158]** But many other beliefs in periods and relations are also found. Thus the idea that the male embryo moves first in the third month, the female in the fourth, appears in the gynaecological treatises.**[159]***On the Nature of the Child* , to which we shall be returning, states that the male foetus takes thirty days at most to form, the female forty-two days, and also maintains that the lochial discharge lasts thirty days for a boy and forty-two days for a girl, a view also expressed in *On the Diseases of Women* book 1.**[160]***On Sevens* , a treatise of admittedly doubtful date, claims that the human seed is "set" in seven days,**[161]** and *On Fleshes* states that it takes seven days for the embryo to acquire all its parts and elsewhere develops other theories of periodicities based on the number seven.**[162]***On Regimen* puts forward an obscure theory

[161] *Hebd.* 1.1.8ff. Roscher, (L) 9.433.3f.

[162] *Carn.* 19 (L) 8.608.22ff., 612.1ff., 5ff.

about the concords or harmonies to which the movements of the developing foetus must correspond.**[163]***Epidemics* book 2 section 3 chapter 17 suggests that the pains in pregnancy occur every third day when there is movement after seventy days, and, further, that they occur on the third day after the fiftieth, and on the sixth after the one-hundredth, and in the second and fourth months.**[164]**

It is not the case that suggestions about such topics as when a male or a female embryo begins to move in the womb invariably take the form of a proposal of a definite number. *Epidemics* book 6 section 2 chapter 25, for instance, probably suggests merely that males move earlier, and develop more slowly after they are born.**[165]** But references to particular numbers of days are very common, even though there is considerable disagreement about which are the significant ones. In some cases we may assume that the proposals are intended to be interpreted flexibly, merely as approximate

suggestions of what may, in general, be expected.**[166]** But in others the theories are stated without qualification. Often the role of symbolic schemata is obvious enough, though, again, in other cases we can do no more than guess on what basis certain numerical relations were proposed. We may, for instance, compare the suggestion that male embryos move in the third month, females in the fourth, with the correlation of male with odd and female with even in the Pythagorean Table of Opposites. Again, it has been suggested that a figure of thirty days for males in *On the Nature of the Child* corresponds to a musical interval of a fourth (two-and-a-half tones, with each tone as twelve days), while one of forty-two days for females is equivalent to a fifth (three-and-a-half tones).**[167]** That is

[163] *Vict.* 1.8 (L) 6.480.21ff., 482.5ff.; cf. 1.26 (L) 6.498.17ff.

[164] *Epid.* 2.3.17 (L) 5.116.12f., 16ff. Cf. *Epid.* 6.8.6 (L) 5.344.10ff., 15ff.

[166] Thus *Nat.Puer.* 18 (L) 7.498.27ff. is concerned, in the first instance, to establish the upper limit to the periods considered, and at (L) 7.500.2ff. states that the rule applies generally and with some variation.

[167] Cf. Lonie 1981a, pp. 192ff.; cf. Delatte 1930.

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conjectural, but more transparently that treatise maintains that the basis for the difference between the sexes here is that the female seed is weaker.**[168]**

Some insight into these theories can be gained from passages where the Hippocratic authors themselves are more tentative or reflective about their proposals. The writer in *On the Eighth Month Child* raises the question of whether women report their experiences in pregnancy correctly. "One should not disbelieve what women say about childbirth," we are told in one context.**[169]** Yet on the difficulties experienced in the eighth month the writer says: "Women neither state nor recognise the days uniformly. For they are misled because it does not always happen in the same way; for sometimes more days are added from the seventh month, sometimes from the ninth, to arrive at the forty days. . . . But the eighth month is undisputed."**[170]**

The writer's own view is that the principal phases of pregnancy consist of periods of forty days, and he is at pains to calculate the beginning of the seventh month with some precision: it begins after 182 days and a fraction, that is, half a solar year.**[171]** He endorses, in the main, the general view of the difficulties of the eighth month but at the same time claims superior,

more exact, knowledge of how to calculate it. It is notable that he does not seek to contradict, so much as to make

[168] *Nat. Puer.* 18 (L) 7.504.24ff.

[171] *Oct.* 4, *CMG* 1.2.1.88.17ff. (*Septim.* 1 [L] 7.436.1ff.). The writer's view that the main phases of pregnancy consist of forty-day periods is set out, for example, at *Oct.* 1f., 5f., and 8, *CMG* 1.2.1.78.6, 80.13ff., 82.21ff., 90.9ff., 22ff., 94.1–14 ([L] *Septim.* 9, 446.15f., 448.21ff., *Oct.* 10, 452.13ff., *Septim.* 2, 436.15ff., 3, 438.14ff., 4, 442.7–22).

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more precise, the traditional conception, including that of the danger to any child born in the eighth month, and indeed he continues to talk of the "eighth month child" even when his own theory is that, strictly speaking, this is inexact.**[172]**

On the Nature of the Child is another treatise that is critical of what women say about their pregnancies, flatly denying that they can be right when they assert that a pregnancy can last longer than ten months.**[173]** When he proposes his theory about the periods required for the formation of the male and female embryo the writer first argues on the basis of the analogy of the equivalent periods taken for the lochial discharge,**[174]** but when he recapitulates "for the sake of clarity" he cites what he calls a piece of research,

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, to support his view. The first consideration he mentions is that on the receipt of the seed the flow of blood into the womb is least, though it subsequently increases (while the reverse is true concerning the lochial discharge) where direct observation of such changes in the flow of blood is clearly out of the question.**[175]** But he goes on to refer to what might have been the far more impressive evidence of miscarriages. "Again, many women have miscarried with a male child a little earlier than thirty days, and the embryo has been observed to be without limbs; whereas those that were miscarried at a later time, or on the thirtieth day, were clearly articulated. So too in the case of female embryos which are miscarried, the corresponding period being forty-two days, articulation of the limbs is observed. Hence both the earlier and the later miscarriages

[172] *Oct.* 2, *CMG* 1.2.1.82.19 and 21; 5, *CMG* 1.2.1.90.18; 10, *CMG* 1.2.1.96.12 ([L] *Oct.* 10, 7.452.10 and 12, *Septim.* 2, 438.10, 8, 446.7).

[173] *Nat. Puer.* 30 (L) 7.532.14ff.: "But those women who imagine that they have been pregnant for more than ten months—a thing I have often heard them say—are quite mistaken" (cf. Aristotle *HA* 584b18ff., 21ff.). The Hippocratic author goes on to identify the source of their error, (L) 7.532.16ff.: "it can happen that the womb becomes inflated and swells as the result of flatulence from the stomach, and the women of course then think that they are pregnant," and it may be too that the menses are interrupted; cf. also (L) 7.534.10ff.

[174] *Nat. Puer.* 18 (L) 7.500.4ff.

[175] *Nat. Puer.* 18 (L) 7.504.2ff., 8ff.

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show both by reasoning and by necessity, that the period of articulation is, for a girl, forty-two days, and for a boy, thirty."**[176]**

What is so striking about this passage is the disparity between the impeccable statement of method, and what the writer provides by way of the results of its purported application. He recognises very clearly that miscarriages would, provided the time of the miscarriage is known, yield telling evidence about the various stages in the development of the human embryo, male or female. Yet what he claims as his result is simply the complete and total endorsement of his theory. His statement of what miscarriages reveal is suspiciously vague and general, and although it may be too much to say that he has no actual evidence at his disposal at all, at least he does not here provide detailed documentation of any single case.**[177]**

Finally, the continuation of the text already quoted from *Epidemics* 2.3.17 shows that, within limits, questions could be raised about some of the periodicities that were proposed. After advancing his theory about pains on every third day when there is movement after seventy days, the writer proceeds: "Should the nine months be numbered from the [last] menstruation or from conception? Do the Greek months amount to 270 days, or is there an addition to these? Does the same apply for males as for females, or the opposite?"**[178]** Yet it is significant

[177] Contrast *Nat. Puer.* 13 (L) 7.488.22ff., which provides some circumstantial detail concerning the writer's observations of what he takes to be an aborted six-day-old embryo discharged by a prostitute owned by a kinswoman. Compare also the examination of the aborted embryo at *Carn.* 19 (L) 8.610.3ff., 5ff.

that even when, as here, certain questions are raised about accepted beliefs, those questions are formulated within the framework of those very beliefs. The writer clearly assumes that pregnancy generally lasts "nine months"; *that* is not in doubt. What is in question, rather, is how the nine months are to be calculated, that is, to put it bluntly, how the presumption of the nine-month period is to be validated.

There is thus a fair degree of disagreement both about what the significant periods in pregnancy and childbirth are and about how they are to be calculated. But that *some* calculation of days for some relations is correct is common ground to many authors. Theories about the periods at which the child born is or is not likely to survive are, in the main, based on popular beliefs which we may suppose to have originated in many cases long before the earliest Hippocratic treatises. The Hippocratic writers, for their part, are often critical of such beliefs, and sometimes they support their criticisms with appeals to what is claimed to be direct evidence. The importance of such empirical support is, we may say, certainly appreciated in principle. Yet in practice, in this context, what the Hippocratic writers offer is often little more than a more or less elaborate rationalisation of popular beliefs. In many cases the criticism is not that some popular assumption is too dogmatic and too precise, but that it is too imprecise—where the Hippocratic writer claims more accurate knowledge of the periodicities in question.

The second main area in which the medical writers develop complex theories of numerical relations concerns the periodicities of diseases, especially of "acute" diseases, that is, those accompanied by high fever. Here less is owed to popular assumptions, or at least there is no good evidence that the development of the classification of fevers into tertians, quartans, and so on antedates the period in which the Hippocratic writers themselves worked, although such a notion is not, of course, confined to them.

As already noted, certain diseases do in fact exhibit marked periodicities, and it is not too difficult to see this as one important and continuing stimulus to the elaboration of Hippocratic theories on the subject. Naturally enough, many writers share the general classification of acute diseases according to their periodicities: there were not

just tertians, quartans, quintans, septans, and nonans, but also semitertians, and as fevers that did not fall into any other category could be termed "irregular,"

, the classification could be made exhaustive. But in addition a wide variety of specific proposals are made concerning complex periodicities, especially doctrines associating groups of even, or of odd, days together. Thus *Epidemics* book 1 chapter 12 states:

> Where paroxysms are on even days, the crisis too is on even days. Where the paroxysms are on odd days, the crisis is on odd days. The first period in those with crises on even days is 4, then 6, 8, 10, 14, 20, 30, 40, 60, 80, or 120 days. In those with crises on odd days the first period is 3, then 5, 7, 9, 11, 17, 21, 27, or 31 days. Further, one must know that if the crisis is on other days than those mentioned, there will be relapses and also it may prove a fatal sign.**[179]**

Offering a theory about the days on which sweating is beneficial in fevers, one of the *Aphorisms* repeats the same sequence of odd days, though adds to these the fourteenth and the thirty-fourth day.**[180]** The treatise *On Humours* recommends that if the paroxysms occur on odd days, the patient should be evacuated upwards on odd days, and that if the paroxysms are on even days, the evacuation should be downwards on even days—although if the periods of the paroxysms are different,

[180] *Aph.* 4.36 (L) 4.514.8ff. Other texts where the emphasis is on odd days are *Aph.* 4.61 (L) 4.524.3f.; *Morb.* 2.41 (L) 7.58.9ff., *Morb.* 3.3, *CMG* 1.2.3.72.14f., *Morb.* 4.46 (L) 7.572.1ff.; and cf. also *Acut.* 4 (L) 2.250.11ff.; *Aph.* 4.64 (L) 4.524.10ff.; *Coac.* 79 (L) 5.600.15f., 142 (L) 5.614.3ff.; *Epid.* 2.5.12 (L) 5.130.14f., 5.15 (L) 5.130.17f., 6.8 (L) 5.134.13ff., 6.10 (L) 5.134.16ff. See Kudlien 1980.

evacuation should be upwards on even days and downwards on odd ones.**[181]***On Diseases* book 4, however, expresses a different view when it sets out to explain why deaths occur on odd days. "Thus the pain happens especially on odd days. Everyone knows that. . . . Those suffering from continuous fever who have been purged on even days have not been over-purged. But those who have been given a strong drug on the odd days have suffered from excessive purgation and many of them have died from this."**[182]**

Elaborate theories are not confined to sequences of odd or even days. *Prognosis* chapter 20, for instance, states

> Fevers have their crises in the same number of days whether the patient survives or dies. The mildest fevers, and those that give the

> surest indications of recovery, cease on or before the fourth day. Those that are the most severe and accompanied by the worst signs cause death on the fourth day or earlier. The first attack of fever ends in this period, the second lasts until the seventh day, the third till the eleventh, the fourth till the fourteenth, the fifth till the seventeenth, the sixth till the twentieth day. In the case of the most acute diseases, the attacks continue up to twenty days, each one adding four days at a time, and then end.**[183]**

[181] *Hum.* 6 (L) 5.486.4ff.

Aphorisms , too, at one point, proposes a mixed theory, where the fourth, eighth, eleventh, and seventeenth days are particularly significant and the ones for the doctor to consider with special care.**[184]**

Some of the more complex theories relating to extended periods are quite fantastical. Yet it is certainly not the case that all that these Hippocratic writers were doing was giving free rein to their speculative imaginations. On the contrary, alongside the apparently dogmatic schemata put forward in some texts, others—especially in the *Epidemics* —show that even while their authors continue to be preoccupied with the problem of periodicities, they were prepared to recognise variations in the patterns of those experienced and to qualify the generalisations they proposed. First, it is worth noting that the detailed case-histories in *Epidemics* books 1 and 3 rarely concern diseases that fall *exactly* into a clearly defined category, such as quartans with exacerbations on every fourth day (calculated Greek style, including both first and last days of each period) or septans on every seventh—even though there are occasions when the case-history incorporates a note, for example, to the effect that the pains generally occurred on the even days.**[185]** Moreover, in the Constitutions in these books plenty of attention is paid to the differences between some individuals' experiences and those of others. Thus in *Epidemics* book 1 chapter 9 we read:

> The circumstances of the crises by which we distinguished them were sometimes similar and sometimes dissimilar. Thus, two brothers who lay near the summer residence of Epigenes fell sick together at the same time. The elder reached a crisis on the sixth day, the younger on the seventh. Both relapsed at the same time, with an intermission of five days.

[184] *Aph.* 2.24 (L) 4.476.11ff.: "The fourth day is an indication of the seventh; the eighth is the beginning of the second week; the eleventh is to be watched since it is the fourth day of the second week; the seventeenth too is to be watched, for it is the fourth from the fourteenth and the seventh from the eleventh." Cf. *Morb.* 2.61 (L) 7.96.5f., *Epid.* 5.73 (L) 5.246.9ff.

[185] E.g., *Epid.* 1 case 1 (L) 2.684.9, *Epid.* 3 cases 3, 10 and 12 of the second series (L) 3.116.12f., 132.4f., 136.13.

> After the relapse they reached a complete crisis together on the seventeenth day. *In most cases* the crisis was attained on the sixth day and, following an intermission of six days, a second crisis was reached on the fifth day of the relapse. *In some* the crisis took place on the seventh day, the intermission lasted seven days, with a crisis on the third day after the relapse. *In others* the crisis occurred on the seventh day, the intermission lasted three days, with a crisis on the seventh day after the relapse. *In others* a crisis took place on the sixth day, the remission lasted six days and this was followed by three days' relapse, a remission of one day, a relapse of one day, and finally the crisis. This happened to Evagon, the son of Daitharses. *In others* a crisis took place on the sixth day, the remission lasted seven days with a crisis on the fourth day of the relapse, as happened to the daughter of Aglaidas.**[186]**

Moreover, the treatise *Prognosis* , which proposes, as we have seen, an intricate theory concerning the periodicities of fevers, goes on to raise certain questions in this connection. "None of these periods," the writer remarks, "can be *numbered in whole days* exactly." Rather, they are like the solar year or lunar month, for neither of them is "such as to be *numbered in whole days* ."**[187]** Apart from this important reser-

vation about the calculation of periods by days, the writer observes that "it is very difficult to distinguish at the beginning between those fevers which are going to reach a crisis in a long period, for they are very much alike in the way they start. But you must pay attention from the first day, and reconsider as each four-day period is added, and thus the way the disease will develop will not escape you."**[188]**

In such texts from the *Epidemics* and *Prognosis* we have impressive testimony both to the doctors' determination to carry through a sustained programme of clinical observations and to the caution and open-mindedness with which they evaluated their data in their attempts to determine the phases of diseases. The outer limits to that open-mindedness are, however, apparent. Practitioners are advised not to jump to conclusions about the nature of the particular case they are dealing with: they are warned to expect that the exacerbations and remissions of different individuals in the same epidemic may vary; al-

though counting the days is the usual method of measuring the periods, they are sometimes told not to assume that periodicities will consist of multiples of whole days.**[189]**

Yet all this excellent advice is given on the basis of the assumption that the periodicities are there to find. They may be hard to identify: many fevers may simply be "irregular." But the presumption is that the periodicities will usually be determinable, and even that complex cycles of exacerbations and remissions will be. The more care and attention the doctors devoted to establishing the times of the crises, the more confident they could feel in their conclusions, not just in particular cases but in general. The grounds themselves of the general theory, however, were not examined critically, or not critically enough, and reflections on the *causes* at work generally presupposed that theory.**[190]** It was enough for the more cautious doctors that periodicities could sometimes be spotted. Meanwhile the more speculative theorists had no compunction in making the most extravagant proposals concerning complex numerological relationships.**[191]**

[189] This point is picked up and elaborated by Galen, for example, (K) 9.870.13ff., 933.12ff., 937.3ff., *CMG* 5.10.1.123.12ff., (K) 17A.246.4ff.

[190] Typical in this area are such suggestions as that quartans are produced by or associated with black bile, tertians and quotidians with other kinds of bile: *Nat. Hom.* 15, *CMG* 1.1.3.202.10ff., 204.8ff., 11ff.; cf. *Morb.* 2.40–43 (L) 7.56.3–60.24; Caelius Aurelianus *De Morbis acutis* 1.108 on Asclepiades.

[191] Later writers sometimes criticised the periodicities proposed by "Hippocrates," as Celsus, for example, did partly on the grounds of the inconsistencies detected between one Hippocratic text and another: see *Med.* 3.4.11ff., *CML* 1.106.25ff., and compare Galen's comments on this issue at (K) 9.868.11ff. and *CMG* 5.10.1.123.12ff., (K) 17A.246.4ff.; at *Med.* 3.4.12, *CML* 1.107.2ff., Celsus quotes the view of Asclepiades that no day was more dangerous to a patient for being even or odd, and at *Med.* 3.4.15, *CML* 1.107.23ff., Pythagorean numerology is singled out for criticism and said to have misled ancient doctors. Later still Caelius Aurelianus, for instance, notes that the periods in epilepsy, for example, are not regular and recommends that treatment should not depend on the number of the days but on changes in the disease, but he nevertheless takes the three-day periods as the starting-point for his discussion and offers advice as to how these are to be recognised: *Morb. Chron.* 1.105, 126.

The Underlying Epistemological Factors

The evidence we have reviewed is enough to show that no simple hypothesis to the effect that the ancients totally failed to make use of measurement will do. But we must now raise the question of the underlying epistemological factors at work. There was, of course, no orthodoxy on the question of the foundations of knowledge in antiquity, whether in the investigation of nature or elsewhere—no one standard set of views shared by all who engaged in that investigation, any more than among those who were more purely philosophical in their interests.**[192]** But how far can we go towards identifying the factors that militated for and against the appeal to measurement?

For Koyré and no doubt many others, the key factor would be the influence of Platonism. To be sure, the dichotomy between reason and perception and the preference for reason over perception—even for reason to the exclusion of perception—have strong roots already in the pre-Socratic period.**[193]** But the theme of the untrustworthiness of perceptible phenomena is associated particularly with prominent statements in Plato, especially the Plato of the middle dialogues,**[194]** where the doctrine takes various forms. The emphasis is sometimes on the simple fact that such phenomena are subject to change,**[195]** but more often also on the further point that particulars bear the predicates they bear in a qualified or relative fashion: what is beautiful in one respect may be said to be ugly in another, appear beautiful to some people but not to others, at one time but not at others, and so on.**[196]**

[192] I have discussed aspects of what follows at greater length in G. E. R. Lloyd 1982, pp. 128ff.

[193] See, for example, Heraclitus fr. 107 ("eyes and ears are bad witnesses for men if they have souls that do not understand the language"), Parmenides fr. 7, Melissus fr. 8, Empedocles frr. 2, 3, Anaxagoras fr. 21, Democritus frr. 9, 11, 125.

[194] See, for example, *Phd.* 65b–c, 79a–c, *R.* 529b–c (in the context of the astronomical programme of the Guardians), *R.* 532a, cf. *Ti.* 28b, 52a–b, *Phlb.* 59a–b. Yet at *Phd.* 74b, 75a–b, perception stimulates the soul to recollect the Forms.

[195] For example, *Smp.* 210e6–211a2.

[196] The classic statement is at *Smp.* 211a2ff., cf. *Phd.* 74b8–9, *R.*479a–b. The secondary literature on these topics is immense, but one of the clearest discussions is Vlastos 1965/1973; see also the pioneering article of Owen

1957/1986.

No one can doubt that Plato's views were extremely influential, and not just among ancient writers who represented themselves as his followers. But it would be nonsense to conclude that the whole of the ancient inquiry into nature was hamstrung by Platonic or Platonising inhibitions about the inadequacy of all perceptible phenomena. That would be to ignore, first of all, that there were plenty of philosophers who took a radically different view from Plato's in the epistemological debate. It is not just that Aristotle (following, in some cases, hints and indications in Plato himself)**[197]** restores an important role to perception and insists that nature is investigable.**[198]** In the Hellenistic period, both major positive schools of philosophy, the Epicureans and the Stoics, took perception to be, in some sense, the ultimate foundation of knowledge.**[199]**

The positions adopted by many of those who were primarily engaged in the inquiry into nature are difficult, sometimes impossible, to specify with any precision, since many of them do not enter the epistemological debate directly nor even necessarily reveal clearly their epistemological assumptions.**[200]** Yet if we consider some of the major

[197] Cf. above, Chap. 3 n. 126, on the *Timaeus* and *Philebus* .

[199] The Sceptics, accordingly, for their part, aimed to undermine perception as well as reason as the "criterion." Among important recent studies of the epistemological debate within Hellenistic philosophy should be noted especially Striker 1974, 1977, Detel 1975, E. N. Lee 1978, von Staden 1978, Burnyeat 1980a, 1984, C. C. W. Taylor 1980, Annas 1980, M. Frede 1983.

figures and investigations in the exact sciences we have mentioned—Eratosthenes and Posidonius in geophysics, Hipparchus and Ptolemy in astronomy, Aristoxenus, Ptolemy, and Porphyry in harmonics, Ptolemy again in optics—there is nothing to suggest that they entertained *radical* sceptical doubts about the value of sense perception for their inquiries—that they believed that perception would provide *no* reliable information at all about what they were investigating.

There are certainly differences in the comparative importance attached to,

and in the comparative use made of, reason and perception, between one theorist and another, between one field of inquiry and another, even between one set of problems within one field of inquiry and another set,**[201]** and many texts—as we saw—draw attention to particular difficulties of observation in particular circumstances. But it is precisely because *normally* observation is *not* subject to such difficulties that they are worth drawing attention to where they exist. It would be pointless for Hipparchus and Ptolemy to criticise their predecessors for their "rough-and-ready" astronomical observations if there were fundamental epistemological reasons for treating *all* astronomical observations as unreliable, including those of Hipparchus and Ptolemy themselves.

Yet even if these points are conceded, other objections or worries about the possible effects of a Platonising influence might remain. There is no difficulty in showing that many ancient researchers do not

[201] On the extent to which, in harmonics, a preference for reason was associated with the positive denial of the validity of the evidence of the senses, see below, Chap. 6 at nn. 37ff.

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ignore nor dismiss the empirical phenomena entirely. But it might still be argued that the search for exact data was inhibited by a general expectation that any data gained from observation will fall far short of the true reality. The problem can be stated simply, but it is of the very greatest complexity, and it would be foolish to try to generalise about the expectations of ancient investigators even within a single discipline. Obviously, those expectations will vary, depending on, among other things, the individual's view of the difficulties encountered in conducting observations, whether with or without instrumental aids, and especially on the confidence he had in his theories.**[202]** Yet—to take our best-documented example again—although Ptolemy often acknowledges inexactnesses in the astronomical data he uses, it is not that he is *indifferent* to their magnitude. It is not that he has a metaphysical principle that allows him to *disregard* such inexactnesses. On the contrary, his concern is always to insist that the inexactnesses he tolerates are minor and fall within the bounds appropriate to the particular problem in question.**[203]**

Paradoxically, perhaps, the very fact that he engaged in some selection and adjustment of his data in the light of his theories reveals *his* expectation that the fit between them will, in general, be a good one. This is true in certain contexts in the *Syntaxis* , but the evidence we considered from the *Optics* is even more striking in this regard. There, in the tables of refraction for the three pairs of media studied, the results are given as correct to within half a degree.**[204]** But they all tally exactly with the underlying general law. Yet this very feature of his account—which shows that Ptolemy has adjusted his

results—*also* reveals that his assumption is that a *perfect* fit between the observed data and the theory is possible, not just a perfect fit between the generalisations derived from the observations and the theory but one between

[202] Cf. further below, Chap. 6, pp. 315ff.

[203] Cf. Ptolemy's frequent appeal to the notion of "negligible difference," both in his astronomy, e.g., *Syntaxis* 3.1 (H) 1.194.10ff., 196.21ff., cf. *Syntaxis* 5.10 (H) 1.394.6, 400.11f., and cf. below, p. 305. on *Syntaxis* 9.2 (H) 2.212.9f., and in his harmonics, *Harm.* 1.4 (9.23ff. D.), 1.16 (39.20 D.), cf. 1.14 (32.20f. D.).

[204] See above, p. 246: note *ad prope* at *Optics* 5.11.229.5, cf. 18.234.2, 21.236.9 L.

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what he represents as the observed results themselves and that theory. Here there are no signs of inhibitions stemming from a belief that the data are bound to prove intractable. On the contrary, this example shows very clearly that the error is, at least on occasion, on the side of expecting, or assuming the possibility of, *too close* a fit between theory and data rather than on the side of the opposite assumption.

Thus far I have concentrated on aspects of the epistemological background that might be thought, or have been thought, to work *against* a realisation of the importance of quantitatively precise data. But one other influential idea that tells, rather, in the opposite direction is the Pythagorean doctrine that "all things are numbers." This was admittedly a highly obscure, at points perhaps even obscurantist, principle. The relationship between "numbers" and "things" is expressed in different, even incompatible, forms in our sources for early Pythagoreanism, for sometimes things are said to *be* numbers, sometimes merely *like* them.**[205]** More important still, the examples cited to illustrate and support the principle include many that have nothing to do with natural philosophical inquiry, as when justice is associated with the number four, or marriage with the number five (the sum of three and two, identified with male and female, respectively).**[206]** Again, we noted that other symbolic associations (not confined to those made by the Pythagoreans) appear to underlie many of the complex numerological relationships found in Greek medicine.**[207]** Moreover, Aristotle reports and criticises overenthusiasm for the number seven: to the reflection that there are seven vowels in Greek, seven notes to the scale, seven

[205] See, for example, Aristotle *Metaph.* 985b27ff., 32ff., 986a2ff., 987a19, b11f., 27f., 1080b16ff., 1083b11ff. On the interpretation of these

reports, see, for example, Guthrie 1962, pp. 229ff., Burkert 1972, chap. 6. As has been emphasised recently by Huffman (in an unpublished paper on "Philolaus and Early Greek mathematics" presented to a conference on Greek mathematics held at Cambridge, England, in May 1984) the attribution of the doctrine that things are numbers is more often an inference from what Aristotle believes the Pythagoreans are committed to, than a direct report.

[206] See, for example, Aristotle *Metaph.* 985b29ff., 1078b22f. Other ancient testimonies and examples are collected and discussed by Burkert 1972, pp. 466ff.

[207] See above, pp. 258ff.

Pleiades, at seven years children lose their first teeth—or at least some do—and that there were seven who fought against Thebes, Aristotle's reaction is to say that such theorisers are like the Homeric scholars who see small resemblances but neglect important ones.**[208]**

While in many contexts the interests shown by Pythagoreans and others in the classification of numbers**[209]** and in proportions, concords, and harmonies**[210]** reflect ethical, symbolic, or aesthetic considerations, in others the theory that "all things are numbers" could and did act as a stimulus to find those numbers, by measurement, in the phenomena. The Pythagoreans, we said, had no monopoly of interest in the numerical relationships investigated in the study of harmonics. But the expression of the concords of octave, fifth, and fourth in terms of the

[208] Aristotle *Metaph.* 1093a13ff., 26ff.

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simple ratios 1:2, 2:3, 3:4 ranked for them, we may be sure, as a paradigm of the application of numbers to things. The exactness here is a matter of the simplicity of the mathematical relationships: the ratios are either multiplicate or superparticular. Yet those ratios had broadly to be confirmed, if not discovered, by reference to measurable data,**[211]** and various investigations involving measurement were attempted, not just on the monochord but also, for example, measuring lengths of pipe or the quantities of water in jars that gave different notes when struck, and even weighing hammers that did so.**[212]**

As is well known, the stories that report some of these inquiries contain many elements of pure fantasy, especially concerning the results that were supposed to have been obtained.**[213]** Yet that does not affect their value to us as evidence for the aims and methods of such investigations. Sometimes the inquiry involves working back from the results expected: thus in the story where predetermined quantities of water are poured into jars, the quantities are *chosen* to *yield* the harmonies. Sometimes the investigation proceeds from what is already given: thus in the story about the hammers, they were clearly not *made* by anyone to give the notes they were supposed to have done. But what the two types of inquiry have in common is the attempt, or

[211] This is true of the principal concords of the octave, fifth, and fourth, even though in the mathematical development of musical theory there could be no question of empirical verification of such ratios as that of the lemma (256:243)—corresponding to a fifth less three tones or to a fourth less two—or of the various theoretical subdivisions of the semitone; see Burkert 1972, p. 385. It is noteworthy that the numerical ratios for the principal concords were common ground not just to Pythagoreans but also to other musical theorists, for example, those working in the Peripatetic tradition: see, e.g., pseudo-Aristotle *Problemata* 19.35.920a27ff., 41.921b1ff.

[213] Cf. further below, Chap. 6 at nn. 37ff.

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the ambition, to reveal the "numbers" in the "things" by *measurement* . Such measurements as were actually carried out in this field may often have been cursory or careless, under pressure from preconceived opinions and reflecting the desire for simplicity.**[214]** Yet whatever else has to be discounted in the stories as we have them, they clearly indicate how the general doctrine that "all things are numbers" could promote quantitative investigations of the phenomena.

Conclusions

To attempt anything like an overview of such a manifold and complex issue as the use and abuse of measurement and the quantitative in ancient science is, no doubt, foolhardy, but some concluding remarks may serve to set out some of the results of our discussion. It is easy enough to see that blanket condemnations—the charge that ancient science is *never* quantitative in character—are well wide of the mark. The ancients' performance in different contexts and at different periods varies, and each field and period must be judged on its own merits, guarding, as always, against what are, in this case, the particularly prominent dangers of distortion that arise from expectations that reflect *our* knowledge of the

eventual successful exploitation of measurement in various domains.

The first fundamental point is the negative one: it is not the case that the epistemological and methodological assumptions at work in the inquiry into nature were always and everywhere hostile to the pursuit of exactness in either of the two forms we are concerned with, that is, (1) the formulation of rigorous theories, and (2) the collection of precise data. Rather, those epistemological and methodological assumptions were, like their actual practice, very much a patchwork quilt of competing and opposing tendencies. But as regards the first form of exactness, the formulation of rigorous theories, we have noted

[214] The powerful attraction exercised by simple numbers is, of course, not a feature confined to ancient science: see, for example, Holton's analysis of Millikan's oil-drop experiments and his comments on Dalton and Mendel, Holton 1978, pp. 25ff., 55–58, 68–70.

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that the application of mathematics to the understanding of natural phenomena of various kinds was one of the most important and fruitful preoccupations of ancient science.

Then as regards the second issue, the collection of precise data, we have seen that while the perceptible phenomena were not usually going to be as exact,

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, as the mathematical theory**[215]** (not without a little "doctoring," at least), the goal of a precise and comprehensive data base was, on occasion, pursued with some vigour. Moreover, it is not just the case that the ancients undertook some precise measurements; they were, at least sometimes, clearly conscious of the applicability of measurement *as an issue* , and Plato, for one, we said, used that, in the *Philebus* , as one of the chief means of classifying sciences.**[216]** They did not just—sometimes—*practise* measurement, but they had the *concept* .**[217]** Nor are the results obtained, in terms of the successful quantitative explanations of the phenomena, negligible, even if many of the successes are the products of the Hellenistic, not of the classical, period.

The criticism of an ancient failure to pursue precision in the data discounts much important work in the exact sciences. In some other areas that criticism is positively wrong-headed, both misrepresenting what is the case and misjudging where the chief weakness of the an-

[215] Thus Aristotle contrasts "mathematics" and "physics" and distinguishes between different mathematical inquiries according to whether the objects dealt with are material or are "said of an underlying subject": see *APo.* 87a31ff., *Metaph.* 982a25ff., 995a6–17, cf. *EN* 1094b11ff., 25ff., and cf. further below, Chap. 6 at nn. 18ff.

[216] As noted above, n. 101.

[217] A distinction may be drawn between measurement used to correct theory, and theory used to correct measurement. In antiquity, measurement is more often subordinated to theory and, as we have seen, measurements are sometimes adjusted to fit theories. The "extraordinary" role of measurements to produce striking anomalies that stimulate a crisis in theory, discussed by Kuhn 1961/1977, pp. 204ff., 211, is more difficult to exemplify from ancient science. Yet where measurements were used to extract the parameters of astronomical models, for example, as in Ptolemy, it is sometimes the case that modifications come to be introduced in the models themselves in response to the data obtained, as, for instance, in the lunar theory and in that of Mercury (see above at n. 95).

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cients' approach lies. Far from being inadequately quantitative, some areas of ancient inquiry were excessively so,**[218]** in part under the influence of the very successes obtained in such fields as harmonics and astronomy. An important recurrent phenomenon in Greek speculations about nature is a premature or insecurely grounded quantification or mathematisation. The excessive elaborations of numerical relations in theories concerning the periodicities of diseases and in embryology are examples of this. Another instance is Galen's attempt to distinguish four different grades of hot, cold, wet, and dry. In various versions of atomism, too, atomic shapes are manipulated in a way that is interesting geometrically, but almost wholly arbitrary. Numbers and geometrical relations could be the key to the understanding of the phenomena, but they were often merely the focus of symbolic attention—as on many occasions, notoriously, in Plato.**[219]** The mathematical rigour of an entire inquiry—as in the casting of horoscopes, to revert to an earlier example**[220]** —could be impeccable, but the inquiry remains with too little purchase, with too little grip, on the phenomena. The appeal to the mathematical often gave a spurious air of certainty, the precise

[219] As, for example, in the discussion of the "nuptial number" in *R.* 546b–d, in the element theory and account of the construction of the world-soul in the *Timaeus* , 31b ff., 35b ff., and cf. *Epinomis* 990e f.

[220] Extravagant numerological speculation is easy to exemplify in astrology, as, for example, in the correlations proposed by Ptolemy,

Tetrabiblos 3.11.129.2–142.15, between the number of years of life and the number of degrees, despite his critical remarks about some traditional methods of calculating these, and cf. his correlations between the seven ages of man and the seven planets, 4.10.204.6ff.

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being confused with the accurate.**[221]** Yet this very feature of some ancient work, the pursuit of exactness where it is *in* appropriate, is itself the subject of critical comment by other ancient authors, as for example, by the medical writers who protest against some bids to turn medicine into an exact science.**[222]**

Yet although the characterisation of ancient science as essentially qualitative stands in need of drastic modification, it has a certain limited validity. Appeal to measurement is rare in dynamics and in element theory, and even where it occurs, *actual* measurements are generally not recorded. In some fields the way the ancients usually formulated the problems directed attention to qualitative, rather than to quantitative, aspects. The instruments available to carry out exact measurements are of varying accuracy (a symptom as well as a cause of the problem), adequate enough for weighing and measuring mediumsized lengths and volumes but not, for instance, for measuring short intervals of time. The example of astronomy shows that when there was sufficient motivation, the ancient Greeks could develop some quite sophisticated instruments,**[223]** but in general the improvements made in measuring instruments were modest. Outside astronomy, the weighing and measuring of the ingredients of drugs was the chief context in which ancient investigators were repeatedly engaged, as a matter of

[222] Cf. above at nn. 134ff. and n. 215, Chap. 3 at nn. 89ff. Compare also Aristotle's insistence that ethics, unlike mathematics, is not an exact study, *EN* 1094b23ff., 25ff.

[223] In astronomy, however, the ancient Greeks produced nothing to compare with the imposing bronze armillaries made by Chinese technologists, the development of which is described by J. Needham 1954– , vol. 3, pp. 339ff.; cf. Needham, Ling, Price 1960.

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course, in measuring procedures, and we may remark, first, that this is a simple application of procedures familiar from the marketplace,**[224]** and, secondly, that this is also an example where a good deal of spurious

exactness is in play.

It is too simple to say that what ancient science needed was a greater appreciation of the value of exact measurement: such a judgement would ignore the point that in some contexts counting and measuring were *over* valued, and some ancient scientists were rightly suspicious of phoney precision. Rather, what was needed was measurement directed, and confined, to determinable issues, or a clearer awareness of the importance of that question of the directedness of measurement—though of course no scientist, ancient or modern, could tell in advance which were the problems that would yield to this method of attack.

One final example will serve to underline that last point. The discovery of the diagnostic value of the pulse, ascribable to Praxagoras of Cos working around 300 B.C. , was undoubtedly of the greatest importance to medicine. Yet already in the generation after Praxagoras, the theory of the different kinds of pulse was brought to a high pitch of elaboration by Herophilus. He and his followers undertook a systematic classification of these according to "magnitude," "speed," "intensity," "rhythm," "evenness," and "regularity."**[225]** He clearly understood that pulse rates vary with age and he distinguished a variety of abnormal pulses, such as the "ant-like,"

μυρμηκίζων

, and the "gazelle-

[224] Some of the tricks practised by the "sellers of purple" are discussed in the Aristotelian *Mechanica* 1.849b34ff.: they include not putting the cord in the centre of the balance, pouring lead into one arm of the balance, and using as a beam wood of varying density, exploiting, for example, the greater weight of wood towards the root of a tree or with knots.

[225] Extensive quotations from Herophilus' own works, including his treatise *On Pulses* , together with comments and criticisms of his theories, are to be found in Galen especially, and a further valuable source is Rufus' *Synopsis de pulsibus* (*Syn. Puls.*). A comprehensive collection and evaluation of all the ancient testimonia are now available in von Staden forthcoming, cf. Pigeaud 1978b. Among the more important texts setting out Herophilus' classification of pulses are Galen (K) 8.592.12ff., 625.7ff., 956.16ff., Rufus *Syn. Puls.* 4 (224.1ff. Daremberg-Ruelle).

like,"

.**[226]** Moreover, it is clear from Galen that the stimulus to develop a systematic, quantitative theory of the pulse came in part from music theory. He reports that "just as musicians establish rhythms according to certain defined sequences of time-units, comparing the *arsis* (upbeat) and the *thesis* (downbeat) with one another, so too Herophilus supposes that the dilation of the artery is analogous to the upbeat, while the contraction is analogous to the downbeat."**[227]** Critics objected that you needed to be an expert in music theory in order to follow Herophilus' explanations.**[228]**

Three remarkable features of this endeavour (which is one that continues down to Galen himself and beyond)**[229]** stand out. First, there is the difficulty of carrying out exact measurements of the short intervals of time in question. According to a report in Marcellinus,**[230]** Herophilus,

[226] See, for example, Galen (K) 8.556.1ff., (K) 9.453.6ff., Marcellinus *De pulsibus* 31 (468–69, lines 429–30 Schöne).

[229] Apart from his debts to Herophilus, Galen draws extensively also on the work of Archigenes, whose ten modes of differentiating pulses are reported by Rufus at *Syn. Puls.* 231.14ff. Galen summarises his own theory, which distinguishes twenty-seven main types of pulse, at (K) 9.435.5ff., cf. 8.453.1ff., 493.1ff., 766.1ff., on which see Deichgräber 1957. As Daremberg 1879, pp. 633ff., noted, the ambition to classify pulses according to music theory was not confined to the ancient world. It was revived in the eighteenth century, for example, by Marquet, the author of a treatise published at Nancy in 1747 entitled *Nouvelle méthode pour apprendre, par les notes de la musique, à connaître le pouls de l'homme, et les divers changements qui lui arrivent depuis sa naissance jusqu'à sa mort* .

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undeterred, invented a special kind of water clock that could be calibrated according to the age of the patient, although we do not know what degree of accuracy he obtained or expected from this. Secondly, there is the evident *ambition* to make the inquiry an exact one, to construct pulse theory on the model of music, the successful mathematisation of harmonics. If the main concords are expressible in terms of simple numerical relationships, why not also the main ratios between the dilations and contractions of the arteries? But, thirdly, it is clear that we have yet another example of premature or insecurely grounded quantification. As in the Hippocratic study of the periodicities of diseases, there is, to be sure, *some* basis for the investigation. But that basis could not sustain the elaborate theoretical superstructure erected upon it. The attempt to reduce the data concerning the pulse to mathematically expressible relations like those of music theory shows how the ancients sometimes exercised considerable ingenuity and

persistence in exploring such possibilities, but it also illustrates how in practice that ambition could turn out to be, in part, misplaced.

Chapter Six— Idealisations and Elisions

In the last chapter I discussed both the positive and the negative features of the use of measurement and the search for quantitative exactness in ancient science. This final chapter will be devoted to further aspects of the problem of the match expected between data and theory, between explanandum and explanation. An element of simplification and idealisation is present in all science: it is only by ignoring certain features of what is given that the underlying relationships governing the phenomena can be revealed. Again, a theory is not held to be refuted when what is predicted on its basis is found to disagree, within certain limits, with observed results. The questions we may pose are: What kinds of simplification did ancient scientists allow themselves? What phenomena did they permit themselves to discount and what constraints did they recognise on that discounting? According to a still highly influential view, that of Duhem,**[1]** the ancient slogan of "saving the phenomena" involved, in astronomy, precisely the production of mathematical theories from which the positions of the heavenly bodies could be predicted independently of any physical considerations. The theories were purely calculating devices; they had nothing to do with any underlying physical realities. Whatever the constraints on the theory from the side

[1] Duhem 1908, 1954b. Aspects of Duhem's interpretation of the ancient evidence are criticised in G. E. R. Lloyd 1978b. See further below at nn. 93ff.

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of the match between predicted and observed positions, there were no constraints at least from the side of the physics of the question, since that was of no concern.

The Development of the Notion of Explanation

The validity of that interpretation of areas of ancient inquiry will be examined in due time, but certain preliminary remarks should first be made on the development of the notion of explanation itself. In a sense myths too, as I noted in Chapter 1, provide explanations—of a sort—of the subjects

they deal with, but only in a sense, and only attenuated explanations. The myths in question range from major quasi-cosmological statements about the origin of the universe or of man's place in it, through particular aetiological accounts, down to "just-so" stories about "how the leopard got his spots." In interpreting these the first important point is a sociological one, namely, the context of delivery. Anthropologists themselves took some time fully to come to terms with the problems of context and intentionality in their material. Many stories do not record what adults seriously believe, only what adults habitually say in response to—and maybe in the hope of blocking—a certain kind of inquisitive questioning, from children, for example, or even from anthropologists. It is not as if we seriously believe that babies are brought by storks or found under gooseberry bushes. It is not as if many of us believe that the century plant does actually bloom only once in a hundred years.**[2]**

But apart from the question of whether such stories are claimed to be *true* , the extent to which they are or contain *explanations* is problematic. They may contain the equivalent of explanations, that is, the answers to "how," "why," or "what" questions. But in two ways especially, they are likely to be defective. First, the problem may not be made explicit, and, secondly, the proposed solution may consist of a set

[2] Cf. in the 1970 edition of the *Encyclopaedia Britannica, s.v.* "century plant": "The century plant is a name given to *Agave americana* from the erroneous supposition that it flowers only when 100 years old."

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of arbitrary assertions, the range of applicability of which is left quite indeterminate. This applies not only to tales told to children (under what circumstances storks bring babies is not discussed) but also to quasi-cosmological myths, about the origin of the world, or of humans or animals, or about how fire came to be used or skills discovered. Under what circumstances Marduk split the primeval water goddess Tiamat to make the sky with its celestial waters on the one side and Apsu, the deep, and Esharra, the "great abode," on the other, just does not occur *as a question:* **[3]** no more does why the stones thrown by Deucalion and Pyrrha became men and women.**[4]** That this happened is simply asserted, and it is understood that this was an exceptional occasion with a special outcome. But why thrown stones do not usually metamorphose is not an issue, though it is known that they do not. That very way of querying the story presupposes a framework of natural causation that became self-conscious and explicit only with difficulty and with time—even though that realisation could and did build on what was, in a sense, already common knowledge, or at least commonly assumed.**[5]**

The emergence of what can begin to be called fully fledged explanations of

classes of natural phenomena is an important new development, though a hesitant one, in early Greek philosophy, with the practice of such explanations preceding the theory. The sequence of ideas that Aristotle reports in *De caelo* 2.13 about the shape and position of

[3] *Enuma Elis* Tablet 4.135ff., Pritchard 1969, p. 67. Compare the translation in Lambert 1975, p. 55: "Bel [i.e., Marduk] rested, surveying the corpse, / To divide the lump by a clever scheme. / He split her into two like a dried fish, / One half of her he set up and stretched out as the heavens. / He stretched a skin and appointed a watch, / With the instruction not to let her waters escape. / He crossed over the heavens, surveyed the celestial parts, / And adjusted them to match the Apsû, Nudimmud's [i.e., Ea's] abode. / Bel measured the shape of the Apsû, / And set up Esharra, a replica of the Eshgalla. / In Eshgalla, Esharra which he had built, and the heavens, / He settled in their shrines Anu, Enlil, and Ea."

[4] Pindar *Olympian* 9.41ff., Apollodorus *Bibliotheca* 1.7.2ff., Hyginus *Fabulae* 153, Ovid *Metamorphoses* 1.395ff. (where it is worth recalling the parenthesis, v. 400: *quis hoc credat, nisi sit pro teste vetustas* ?).

[5] Cf. G. E. R. Lloyd 1979, pp. 49ff.

the earth and on the question of why it does not move illustrates both the advances and the limitations of pre-Socratic natural philosophical accounts, even if it is evidence that must be used with caution.**[6]** For one thing, it was Aristotle who chose to present these ideas as a *sequence* of replies to the same set of questions, and that may well distort the original context in which they were proposed. Even so, although it would be quite wrong to represent later theories as progressively more sophisticated (let alone truer or in some sense more correct) than earlier ones, some of the *constraints* on what counts as an answer appear to be grasped more fully as time goes on.

Three features are worth remarking very briefly. First, there is the phenomenon of the *regression of the explanandum* . A common suggestion was that the earth does not move because it is supported on something, such as water (according to Thales) or air (as in Anaximenes).**[7]** That resolved one difficulty by raising another: what keeps the water, or the air, itself in place—a point that was evidently appreciated by Aristotle and may already have been by Anaximander.**[8]**

Secondly, in Anaximander's suggestion—that the earth does not move because it is equally balanced on all sides and there is no reason, then, for it to move in one direction rather than in any other**[9]** —we

[6] Aristotle *Cael.* 2.13.293a15–296a23.

[7] Aristotle *Cael.* 294a28ff. (Thales), 294b13ff. (on Anaximenes, Anaxagoras, and Democritus, who are said to appeal to the flatness of the earth as the explanation of its being at rest: it does not cleave the air beneath it, but settles on it like a lid).

[8] See Aristotle *Cael.* 294a32ff. It seems likely from the evidence in Aristotle *Ph.* 204b24ff., together with Simplicius *In Ph.* 479.32ff., that Anaximander arrived at his cosmological principle, the Boundless, in part by reflecting on the difficulties presented by any view—such as Thales' doctrine of water—that started from a single determinate substance. If so it is possible—though of course far from certain—that a similar line of reasoning led Anaximander to his radically different solution to the problem of the earth being at rest, in which he appealed not to any underlying support but to its being "equally balanced" on all sides (see next note). The proponents of the view that the earth does not move because it rests on air like a lid appear to have argued that the air itself cannot move for lack of room, *Cael.* 294b19ff., 25ff.

[9] *Cael.* 295b10ff., cf. Hippolytus *Refutatio Omnium Haeresium* (*Haer.*) 1.6.3.

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have an example of *suspending* some of the commonly assumed data. A clod of earth, as Aristotle was prosaically to insist, moves in a certain direction, "downwards." Aristotle, with a spherical earth, defined that as towards the centre of the universe, deemed to coincide with the centre of the earth.**[10]** That answer was not available to Anaximander, who thought the earth flat.**[11]** But then in his case that truth about pieces of earth has to be assumed *not* to apply to the earth as a whole, for his suggestion to be an answer to the problem of why the earth does not move.

Thirdly, we find in the same chapter an example of the *denial of the data* that are supposed to generate the problem. The full motivation of the suggestion that Aristotle ascribes to certain Pythagoreans, namely, that the earth is like a planet,**[12]** is unclear and controversial, but the effect of the suggestion is to make the earth move in space. The question "Why does the earth not move?" thus gets answered by denying the assumed fact: "But it *does* move"—though we evidently have another case of the regression of the explanandum, since *how* it moves and how, on the hypothesis of its movement, other phenomena are to be accounted for involve a series of other problems a stage further back.

These first attempts to resolve questions concerning the position of the earth

may look indistinguishable from myths, or at least subject to criticisms that are similar in kind to those I made of the types of

[10] See above, Chap. 4 at n. 79.

[11] See pseudo-Plutarch *Stromateis* 2, Hippolytus *Haer.* 1.6.3, Aetius 3.10.2.

[12] *Cael.* 293a21ff.: the earth is one of the "stars" and makes night and day as it travels round the centre in a circle. At the centre itself is the Central Fire, and Aristotle reports that the Pythagoreans held that fire is more honourable than earth and so occupies this honourable position (*Cael.* 293a30ff.). The Pythagoreans further postulated an invisible "counter-earth" and held that this and maybe other invisible bodies accounted for eclipses of the moon being more frequent than those of the sun (that is, presumably, as seen at any given position on earth) (*Cael.* 293b23ff.). Aristotle himself criticises them for introducing the counter-earth merely to bring the number of the heavenly bodies up to the perfect number ten (*Metaph.* 986a8ff.) and for forcing the appearances to fit their own preconceived opinions (*Cael.* 293a25ff.).

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explanations that are offered in myths. But apart from the well-known point that the philosophers' accounts are naturalistic ones,**[13]** they are in principle subject to open challenge. A new suggestion on an old topic implicitly claims superiority to others in the field and has, accordingly, to give an account of itself. It is in that crucible of debate on contested issues that clearer working notions of what will count as an explanation, and of what an explanation should be, come to be elaborated.

For the first more explicit discussions of that topic we have to wait until Plato, though several of the Hippocratic writers made, rather more incidentally, important contributions to the understanding of such distinctions as that between causal and merely coincidental factors.**[14]** Two of the key ideas for which Plato himself appears to have been responsible are, first, the explicit distinction between necessary condition and cause or explanation, and, secondly, the more general contrast between essence and accident. The first distinction is made in the *Phaedo* , where reference to what is true merely of the material conditions of a situation (without which, to be sure, it would not be the situation it is) is contrasted with reference to the

αἴτιον

, which must specify some good.**[15]** The further point here, that explanation must be

[13] Cf. Farrington's much-quoted dictum 1944–49/1961, p. 37: "What Thales did was to leave Marduk out."

in terms of what a thing is for or the good it serves—that is, that explanation must be teleological—was fraught with significance for the future, and we shall be returning to it later.**[16]**

The second, more general distinction between essence and accident is crucial for the theory of Forms but is present already in the Socratic search for definitions. The *Euthyphro* puts it that definition is directed at the

οὐσία

, what the thing really is, rather than at the

πάθος

, that is, some attribute that it may happen to possess.**[17]** The frequent insistence in the Socratic dialogues on the *equivalence of extension* of definition and definiendum provides one of the clearest early contexts for a demand for an exact match between a *logos* and that of which it is the *logos* . Even though in practice, in the natural sciences, the distinction between essence (or the lawlike) and the accidental will often be problematic and hard to apply, once some such distinction is available it can be appealed to in attempts to determine what, in the phenomena under review, can and should be discounted.

These points are all very familiar. My aim in recalling them is simply to stress the moral they convey, that an explanation—in science or anywhere else—must focus on *certain* aspects of the phenomena in preference to others (causes, not preconditions) and to the exclusion of yet others (that is, must focus on the essential, not the accidental).

Mathematics and Physics in Plato and Aristotle

Further pressure positively to discard certain features of the phenomena comes from the side of the model of mathematical knowledge—it, too, prominent in Plato. The nature of mathematical truths and of the objects that mathematics studies had become, already by Aristotle's time, topics of

intense controversy.**[18]** Where Platonism construes mathematics as to do with separate intelligible objects and

[16] See below, pp. 319ff.

[17] Plato *Euthphr* . 11a.

[18] In *Metaphysics* M and N especially, Aristotle engages in sustained debate with Plato, with Pythagorean positions, and with those of his own contemporaries Xenocrates and Speusippus. See Annas 1976 and the papers collected in Graeser, ed., 1987, especially.

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accepts and insists that the truths of geometry, for instance, are never unqualifiedly instantiated in *physical* objects (the diagram on the blackboard, for example), Aristotle argued that mathematics had no *separate entities* as its objects: mathematics studies certain features *of physical* objects taken in abstraction from certain others, namely, the features that make them the physical objects they are.**[19]** Mathematical truths are, then, truths about the mathematical properties of physical objects. Indeed, it has recently been argued, with some force and sophistication,**[20]** that Aristotle does not merely *not deny* , he even requires that there are physical straight lines that fully and perfectly instantiate the geometrical truths about straight lines. It is true that the line drawn in chalk on the blackboard will not do as an example, nor even its outer edge, but, then, it would be wise to say that they are not straight lines. The truths about straight lines will nevertheless be instantiated in *any* of a number of straight lines that are present in any physical object.**[21]** That interpretation is disputed, but at least it can be agreed that there is no need for Aristotle to say that in principle it is impossible for physical objects to instantiate mathematical truths; they certainly will not fail to instantiate truths of arithmetic,**[22]** and he certainly has some perfect spheres—in the heavens.

Whatever the disagreements between Plato and Aristotle in the philosophy of mathematics, both held that mathematics is exact, and that point is fundamental, even though it requires as a gloss that pure mathematics also has to admit approximations in certain contexts (for the values of surds, for example).**[23]** But in a bid for exactness, applied

[19] See especially *Physics* 2.2.193b22ff., 24ff., 194a9ff.

[20] See Lear 1982: contrast I. Mueller 1970/1979 and 1982a, pp. 70ff.; Annas 1976, pp. 29ff.; and cf. Annas 1987; Hussey 1983, pp. 176ff.

[21] Cf. Lear 1982, pp. 175ff., 180f. The possibility that some early Greek *definitions* of a straight line are based on or derived from the physical law of the rectilinear propagation of a ray of light is discussed by Mugler 1957 and 1958.

[22] On Aristotle's philosophy of arithmetic, see Lear 1982, pp. 183f.; J. Barnes 1985; Mignucci 1987. Aristotle himself does not, however, draw attention to differences between arithmetic and geometry in the relevant connection.

[23] Cf. Archimedes' extraction of a value for *p* in his *Dimensio circuli* . InPtolemy's *Syntaxis* , for instance, approximations are certainly just as prominent in the purely mathematical parts of his calculations as they are in the adjustments made to observational data: cf. G. E. R. Lloyd 1982, pp. 153–59.

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mathematics too will discard, even for the Aristotelian, some, at least, of the physical aspects of the phenomena.**[24]** Although the precise conditions under which the procedure called abstraction,

ἀφαίρεσις

, can be carried out are controversial, *some* discarding under *some* , more or less rigorous, conditions is clearly involved.**[25]** It will not matter if the line in the diagram is not straight or is not a foot long, for the geometer will say: take the line as straight. And if, in fact, it is not so, nevertheless, as Aristotle put it, the falsehood does not lie in the premises.**[26]**

"Saving the Phenomena"

After these rather cursory preliminaries concerning the philosophical background, we may now turn to our principal concern, the kinds of idealisations found in the ancient inquiry into nature. We are told by Simplicius that Plato set as a problem the saving of the apparent wanderings of the planets, by means of regular, orderly—we are to understand, circular—motions.**[27]** But the question of the conditions un-

[24] Thus at *Metaph.* 1078a14ff., harmonics and optics study their objects not *as* sight or *as* sound, but as lines and numbers, and, he adds, "similarly with mechanics," though at *Ph.* 194a11ff., optics, for instance, is contrasted in turn with geometry as concerned with a mathematical line but not *as* mathematical but *as* physical. The more that is discarded, the more exact

the study is: see *APo.* 87a31ff., *Metaph.* 982a25ff.

[25] On Aristotelian abstraction see, for example, Philippe 1948; I. Mueller 1970/1979, pp. 98ff.; Lear 1982 (who gives a sophisticated formal analysis of the *qua* operator as a predicate filter); and cf. Cleary 1985; Annas 1987; Mignucci 1987.

[26] See *APr.* 49b34ff., *APo.* 76b39ff., *Metaph.* 1078a17ff., cf. 1089a21ff.; see Lear 1982, pp. 171ff.

[27] Simplicius *In Cael.* 488.18ff., 492.31ff. (the latter passage specifies circular motions explicitly). In the first text Simplicius gives Sosigenes as his authority and he has just referred to Sosigenes' use of Eudemus' *History of Astronomy* . But who first formulated the problem for astronomy in exactly these terms is unclear: neither Plato nor Aristotle speaks of "saving" the "phenomena" as such, though Aristotle, for instance, refers to saving a "hypothesis" at *Cael.* 306a29f.

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der which it could be considered that certain phenomena *had* been *saved* — or that an adequate account had been given—is an issue not just for the Platonic or Platonising tradition, and not just for astronomy.**[28]** We find a variety of expressions used in a number of contexts: saving the phenomena or appearances (

τὰ φαινόμενα

; cf. also

φαντασίαν

)**[29]** and also saving (

σώζειν

or

διασώζειν

or

ἀποσώζειν

) what arises from them (

τὰ ἐκ τῶν φαινομένων

)**[30]** or the occurrences (

τὰ

συμβαίνοντα

)**[31]** or a variety of other explananda,**[32]** including even generation and destruction themselves**[33]** or such a preconception (

πρόληψις

)**[34]** as a certain notion of unity or the One.**[35]** Unfortunately, how-

[28] Since Duhem 1908 the literature on the topic of saving the phenomena has been prolific: see especially Mittelstrass 1962, 1979, Wasserstein 1962, Krafft 1965, Sambursky 1965, G. E. R. Lloyd 1978b, and, most recently, A. M. Smith 1981 and 1982. Although in his 1981 paper Smith categorises the metaphysical assumptions in the background too readily and too loosely as Platonic, he draws attention both to the role of the prior selection of the salvanda ("what needed saving was what was deemed salvageable," p. 80) and to that of implicit appeals to the principles of uniformity and economy in the "salvations" proposed. Once the anomalies that provided the main problem had been shown to be explicable in terms of uniformities or regularities, they could be considered not exceptions to, but evidence of, order ("Anomaly was not so much a traducing as a token of real order," p. 99).

[29] E.g. Proclus *Hyp.* 5.178.13ff.

[30] E.g. Proclus *Hyp.* 5.156.23f.

[31] E.g. Philoponus *De opificio mundi* 3.3.115.22ff.

[32] Often what is to be "saved" (in the sense of "accounted for") is an (apparent) irregularity, as for instance at Simplicius *In Cael.* 509.18f., cf. 507.10ff., *In Ph.* 292.17f., 21ff. But sometimes what is to be "saved" (in the sense, rather, of "preserved" or "maintained") is a *regularity* , as for example at Proclus *Hyp.* 2.30.22ff.

[33] As, for instance, at Simplicius *In Ph.* 240.24f.

[34] Simplicius *In Ph.* 93.28f.

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ever, many of the explicit references to such slogans (generally in late authors) are vague and leave the requirements on the "saving" indeterminate;**[36]** our best policy is, rather, to study the actual practice of Greek investigators at work, and we may start with some fairly straightforward cases from the exact sciences.

Harmonics

Take, first, harmonics. I mentioned before**[37]** that the apocryphal stories about Pythagoras' discovery of the numerical expressions of the principal concords contain many fantastic elements, not least that the results reported for the tests that he is supposed to have undertaken cannot, in several cases, have been obtained in practice.**[38]** Yet the stories are again important for our inquiry here, since they convey a clear, if implicit, grasp of the principle of *varying the conditions* of a test in order to isolate the relevant factors producing the result. Pythagoras, in one story, passes a smithy and—if we are to believe our sources**[39]** —hears hammers striking concords. He first asks the smiths

[36] Pace Sambursky 1965, p. 5. It is clear from Theon (e.g., 175.1ff.), and from Proclus (e.g., *Hyp.* 2.34.11ff., 38.10ff.) especially, that in astronomy more than one mathematical model may provide a basis for some "saving" of certain phenomena. Yet Theon makes it plain that he is not satisfied with any but the true natural/physical account (177.21ff., 188.8ff., see G. E. R. Lloyd 1978b, pp. 217ff.), and Proclus too demands, so far as possible, true explanations of astronomical phenomena (*Hyp.* 7.238.13ff.) and claims that Plato had grasped these (cf. *In Ti.* 3.96.31f., and for other evidence of realist assumptions in Proclus' astronomy, see G. E. R. Lloyd 1978b, pp. 208–11).

[37] See above, Chap. 5 at n. 213.

[38] See Burkert 1972, p. 375 and n. 23, on the impossibilities involved in some of the alleged experiments discussed already by Mersenne. Ptolemy *Harm.* 1.8 (16.32ff. D.) notes some of the difficulties in some of the trials that purported to reveal the principal concords.

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to exchange hammers, but that does not make any difference to the sounds the hammers make. So it is not the strength of the smith that counts. He then weighs the hammers and—according to our sources—obtains his result—even though this is impossible: the note will vary with the anvil, not the hammer.**[40]**

Those who actually engaged in the study of harmonics (as opposed to merely fantasising about the discoveries of Pythagoras) disagreed about how much of the phenomena to discount, and the epistemological debate, extensively reported in Porphyry especially,**[41]** is sometimes conducted in rather simplistic terms, as if it were a matter merely of deciding whether reason or perception is the ultimate criterion.**[42]** At

[40] The story of Hippasus constructing bronze disks to yield the harmonies is closer to what is possible: see schol. *Phd.* 108d, and cf. Raasted's discussion, 1979, mentioned above, Chap. 5 n. 212, relating this to the story of the hammers.

[41] *In Harm.* 25.3ff. D.; cf. Ptolemy *Harm.* 1.8 (19.16ff. D.).

[42] See, e.g., Ptolemais of Cyrene, quoted by Porphyry *In Harm.* 25.9ff. D., where three groups are distinguished: (1) those who "preferred" reason; (2) those "instrumentalists" who preferred perception; and (3) those who used both. Some of the "Pythagoreans" are here categorised as belonging to the first group, though Pythagoras and his "successors" are in group 3 (cf. Didymus in Porphyry *In Harm.* 26.15ff. D., who suggests that the Pythagoreans preferred reason but used perception with regard to the starting-point of the inquiry only, and cf. Porphyry's own opinion concerning the Pythagoreans at *In Harm.* 9.1ff., 15ff., 33.5ff. D.). As Barker has pointed out, e.g., 1981b, pp. 10f. and 16, many of those who were said to value reason more highly than perception did not totally reject the latter (cf. Aristoxenus at *Harm.* 2.33 and Didymus in Porphyry *In Harm.* 28.12ff. D., who point out that while the geometer can take what is not straight as straight, the musician cannot take what is not a fourth as a fourth). The point was often, rather, that perception cannot make fine discriminations. Nor can it settle the dispute as to whether or not the octave, fifth, and fourth are exactly six tones, three-and-a-half tones, and two-and-a-half tones, respectively, nor the question of whether or not either the tone or the semitone can be divided exactly into two equal intervals.

In one of the opposing traditions, that of Aristoxenus, the claim was that the unit of measurement must be something identifiable by perception. Thus Aristoxenus *defines* the tone as the difference between the fifth and the fourth, both concords immediately apprehensible to perception—whereas to thePythagoreans the tone may, rather, be defined mathematically as the difference between sounds whose "speeds" stand in a ratio of 9:8. Whilst for the Pythagoreans musical relations are to be expressed as ratios between

numbers, for Aristoxenus musical intervals are construed on the model of line segments and their interrelations investigated quasi-geometrically: cf. Barker 1978b, p. 4, 1981b, p. 3.

Each of these two major traditions faced its own corresponding difficulties or anomalies. Aristoxenus found it impossible, in practice, to carry through his programme of founding music theory on the basis solely of an appeal to what can be heard, on the principle that "what [the voice] cannot produce and [the ear] cannot discriminate must be excluded" from the sphere of useful and practically possible musical intervals (*Harm.* 1.14). In particular the principle of concordance, on which his theory relies, will not allow the construction of intervals smaller than the semitone (though he attempts to discuss these, e.g., at *Harm.* 1.21) (cf. Barker 1978a, pp. 15f.). Conversely, as I point out in my text, one problem for the Pythagoreans was that since 8:3 is neither multiplicate nor superparticular, they were led to ignore or deny that the interval of the octave plus a fourth is a concord—and similarly with the double octave plus a fourth.

For Ptolemy and Porphyry, who distance themselves from both the earlier Pythagoreans and from the Aristoxeneans, the chief problems connected with perception are identified as: (1) that different observers obtain different results (e.g., Porphyry *In Harm.* 18.12ff. D.); (2) that instruments may be unreliable (e.g., Ptolemy *Harm.* 1.8 [16.32ff. D.], Porphyry *In Harm.* 119.13ff. D., Ptolemy *Harm.* 2.12 [66.6ff. D.]); and (3) that perception cannot give the exact measure of very small intervals (e.g., Ptolemy *Harm.* 1.1 [4.13ff. D.], 1.10 [21.25ff. D., 24.20ff. D.], Porphyry *In Harm.* 20.12ff., 129.18ff. D., and cf. 75.25ff. D., quoting the pseudo-Aristotelian *De audibilibus* , and 80.22ff. D., reporting Aristoxenus). More generally the "rough-and-ready" character of perception is often referred to, e.g., Porphyry *In Harm.* 16.13ff., 17.6ff., 18.9ff., 19.2ff., 21ff. D.

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one extreme there were those who sought to reduce the subject to number theory: some, we are told, maintained that since 8:3 is neither a multiplicate ratio (like 2:1 or 4:1) nor superparticular (like 3:2 and 4:3),**[43]** the interval of an octave plus a fourth cannot *be* a concord, even if it sounds like one.**[44]** Yet to that Theophrastus pertinently remarked

[43] A superparticular, or epimoric, ratio is defined as $n + 1 : n$, where n is a positive integer greater than 1.

[44] See, for example, the discussion in Ptolemy *Harm.* 1.5f. (11.5ff., 13.1ff. D.), and in Porphyry *In Harm.* 95.25ff., 105.12ff., 124.4ff. D., and cf. Barker 1981b, pp. 9ff.

that what is heard is not a *number* , even if concords are expressible numerically.**[45]**

Whatever their other disagreements, and there were plenty of these, the main practitioners—Aristoxenus, Ptolemy, and Porphyry, for instance—agreed in taking harmonics to be concerned with certain audible phenomena, not *just* with mathematical relations or number theory. Even in the mathematical, Pythagorean tradition represented by such a work as the Euclidean *Sectio canonis* , the main concords—octave, fifth, and fourth—are assumed as given to perception.**[46]** Aristoxenus takes harmonics to deal with the principles of melody, especially the theory of scales and keys,**[47]** and Ptolemy specifies that it is concerned with the differences in the pitch of sounds.**[48]** But even for an Aristoxenus or a Ptolemy, aspects of the audible phenomena are to be discounted.**[49]** The subject matter does not include the volume or magnitude of the note, nor its timbre or quality.**[50]** Again, it is indifferent to the investigator whether he studies wind or stringed instruments. If he is investigating pitch on the monochord, for example, the thickness of the string, its material, and its tension are all irrelevant (though he knows, of course, that if these are altered, so too will be the pitch of the note);**[51]** the only data he is concerned with are the lengths that correspond to certain notes.

[45] An extensive quotation from Theophrastus' lost work *On Music* is given by Porphyry *In Harm.* 61.22ff. D., discussed by Barker 1977. See in particular 62.9ff. D.

[46] See Barker 1981b, pp. 7, 10f., and 16.

[47] Aristoxenus *Harm.* 1.1.

[48] Ptolemy *Harm.* 1.1 (3.1f. D.).

[49] Here, as elsewhere, a distinction may be drawn between discounting some aspects of the phenomena as irrelevant (as is the case with the magnitude of the note) and the discounting of others because the investigator assumes an ideal situation (where again we may contrast, for example, Ptolemy's recognition of the importance, for his purposes, of *securing* an ideal string, *Harm.* 1.8 [17.7ff. D.], cf. n. 51 below, with the *assumption* of an ideal string in, for instance, the Euclidean *Sectio canonis*).

[51] The effects of variations in the thickness, humidity, density, and evennessof strings are discussed, for example, by Ptolemy *Harm.* 1.8 (17.7ff., 27ff., D.), 1.11 (26.15ff. D.), Porphyry *In Harm.* 121.2ff., 133.28ff.

D., Nicomachus *Harm.* 6.246.22ff., cf. Barker 1981b, p. 8.

Optics

In theoretical**[52]** optics, too, there were disagreements about such physical issues as whether the visual ray departed from the eye or travelled to it from the object seen,**[53]** and whether light was a movement,

κίνησις

(or transport,

φορά

) or should be interpreted as an actuality,

ἐνέργεια

, or a tension,

τάσις

.**[54]** Divergent positions were also maintained on the further fundamental question of whether visual—or light—rays form a continuum (as, for example, Ptolemy insisted)**[55]** or are discontinuous (as appears to be assumed in Euclid's *Optics*),**[56]** and this in turn affected beliefs concerning how far the programme of geometrising optics could be carried through and on the constraints on such a programme. Since it is quite clear that in the *Elements* Euclid assumes that geometrical magnitudes are infinitely divisible,**[57]** it was presumably not for purely *geometrical* reasons that he would have departed from that assumption for optical phenomena in his *Optics* , but, rather, for reasons to do with problems connected with the visibility of objects at a distance.**[58]** Yet some geometrisation of optics is common ground to most investigations of perspective, reflection, and refraction—including those of both Euclid and Ptolemy—to the extent at least that it was assumed, first, that visual/light rays can be treated as straight lines,**[59]** and, secondly, that for some purposes the eye

[55] At *Optics* 2.50ff., 37.4ff. Lejeune, Ptolemy insists, against Euclid, that the visible flux is a continuum: cf. also *Optics* 2.48, 35.18ff., 36.5ff. L.

[56] Euclid *Optics* Definition 3, 2.7ff., Propositions 1, 3, and 9, 2.22ff., 4.26ff., 16.7ff., and cf. Theon's *Opticorum recensio* (*Opt.Rec.*) introduction 146.18ff. The interpretation of *Optics* Proposition 3 especially is, however, disputed: see Brownson 1981, p. 174.

[57] There were, however, those who denied this assumption and held that geometrical magnitudes are made up of indivisibles, notably, in the Hellenistic period, the Epicureans, and cf. also the pseudo-Aristotelian treatise *On Indivisible Lines:* see, most recently, Sedley 1976b.

[58] This is the issue in Euclid *Optics* Proposition 3, 4.26ff., especially.

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can be considered as a point, the vertex of the visual cone.**[60]** Moreover, we have good grounds for supposing that the second of these assumptions was clearly recognised by some *as an idealisation* —since on certain occasions the fact that vision takes place not from a point but from a certain area was acknowledged. Archimedes, in particular, provides a sophisticated discussion of the allowance that has to be made for this in the context of his determination of the angular diameter of the sun in the *Sand-Reckoner* .**[61]**

Statics and Hydrostatics

As a third example we may take statics. Although here Archimedes does not state all his assumptions, he evidently discounts, for the purposes of his investigation of the lever, such factors as the possible variation in the material constitution of an actual metal bar and, more

[61] Archimedes *Aren.* 1.10 (HS) 2.222.3ff., on which see, for example, Lejeune 1947–48, Shapiro 1975, pp. 82f. Cf. also the discussion of binocular vision in Ptolemy *Optics* 2.27ff. (26.18ff. L.), and 3.25ff. (102.13ff. L.) with Lejeune 1948, pp. 130ff., 145ff.

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importantly, that the movement of a bar about a fulcrum will be

accompanied by friction.**[62]** Similarly, in hydrostatics he stipulates explicitly that the fluid be perfectly homogeneous and totally inelastic.**[63]** Moreover, in his investigation, in the second book of *On Floating Bodies* , of the conditions of stability of segments of paraboloids of revolution of varying shapes and of varying specific gravities in a fluid, he assumes that he may talk, ideally, of the *centres of gravity* of plane segments of geometrical figures, as well as of the paraboloids themselves.**[64]**

These are, as I noted, on the whole comparatively straightforward cases, and they are, of course, among the most commonly cited examples of the successes of Greek science. There has, to be sure, been much, rather laboured, discussion of the possible circularity of the argument in Archimedes' statics—of the relationship between the first postulate, which states that "equal weights at equal distances are in equilibrium,"**[65]** and the law of the lever subsequently demonstrated, on its basis, in propositions 6 and 7 of book 1 of *On the Equilibrium of Planes* .**[66]** Yet to the charge of circularity it might be countered that of course the law of the lever is in *some* sense presupposed at the beginning, but that is no objection: there is no vicious circularity but, rather, a quite unproblematic, indeed unavoidable, mutual entailment here between postulates and subsequent propositions.**[67]** That point aside, the type of idealisation involved in the studies we have consid-

[62] Archimedes' assumptions are set out in the first book of *De planorum aequilibriis* (*Aequil.*) (HS) 2.124.3ff.

[63] Archimedes *Fluit.* 1 Postulate (HS) 2.318.2ff.

[64] Archimedes *Fluit.* , e.g., 2.2 (HS) 2.350.11ff., 27ff., cf. 1 Postulate 2 (HS) 2.336.14ff., 1.8 (HS) 2.338.26ff., 1.9 (HS) 2.342.15ff., *Aequil.* 1 Postulates 4f. (HS) 2.124.13ff., 16ff. As to whether the postulates in *Aequil.* and *Fluit.* can be thought of as defining implicitly the notion of centre of gravity, see Stein 1931, but contrast Suppes 1981, pp. 207ff., cf. also Schmidt 1975.

[65] Archimedes *Aequil.* 1 Postulate 1 (HS) 2.124.3f.

[66] *Aequil.* 1.6 (HS) 2.132.14ff. (for commensurable magnitudes), 1.7 (HS) 2.136.18ff. (for incommensurable ones), on which see Mach 1893, pp. 13f., cf. 1960, pp. 19f.; Duhem 1905–6, vol. 1, pp. 9ff.; contrast Knorr 1978b, p. 185; Suppes 1981, p. 212 n. 3.

[67] Cf. the discussions of Knorr and Suppes cited in the last note.

ered so far is uncontroversial, indeed, not just uncontroversial but the fundamental factor on which the advances in understanding that were made depended. It is only by thinking away some of the features of the phenomenal situation that the underlying, mathematically expressible relations can be revealed.

Dynamics

We can go further: it was partly because of a failure to think away *sufficient* of the factors in the phenomenal situation that the ancients, Aristotelians and anti-Aristotelians alike, failed to arrive at satisfactory resolutions to the problems in the field we identify as dynamics. Aristotle himself, for instance, *argues* , as we saw in Chapter 5, that motion *must* be through a medium; in some of the texts setting out the proportionalities of natural motion he has the express purpose of *disproving* the void.**[68]** Philoponus attacked the Aristotelian position on the role of the medium and maintained that it acts purely as a resistance to the moving object.**[69]** Yet Philoponus, like Aristotle, assumed that weight is *one* of the factors that determine the speed of a freely falling object and, moreover, not only that it does so in a plenum but also that it would do so in a void.**[70]** But if you take as your explanandum, or as one of your explananda, motion *through a medium* , this is bound to prove a major stumbling block to analysis, if only because of the difficulty, indeed the impossibility, of *quantifying* the factor that corresponds to the density of the medium—a problem that is expressly remarked on by Philoponus.**[71]**

[68] Aristotle *Ph.* 4.8.215a31ff., b12ff.; see above, Chap. 5, pp. 217ff.

[69] Philoponus *In Ph.* 647.9ff., 681.10ff., 682.29ff., especially, and cf. above, Chap. 5 at n. 29.

[70] Philoponus denies that an actual continuous void exists in nature: see Sedley 1982a and forthcoming, Furley 1982. We may contrast the position of the Epicureans, who both asserted the void and maintained that in the void heavy and light atoms move with equal speed "as quick as thought," Epicurus *Ep. Hdt.* 10.61, cf. 48, and cf. Furley 1967, pp. 121ff.

[71] Philoponus *In Ph.* 683.1ff., see above, Chap. 5 n. 33.

Astronomy

It would be easy to conclude, on the basis of some of the examples

considered so far, that the problem with ancient science is more often too *little* abstraction than too *much* . Yet that would be premature. If we turn to astronomy, we see how much more complicated the issue is. Early on in the development of Greek astronomical model-building we find what appears to be a striking example of discounting part of the known data. According to Simplicius,**[72]** the differences in the apparent brightness of particular planets had already been taken, in Eudoxus' day, to indicate that the distance of each planet from the earth is not constant. Simplicius reports that Eudoxus and Callippus failed to deal with the problem and, indeed, that other astronomers down to Autolycus of Pitane also did—not that Autolycus himself was successful in resolving the difficulty.**[73]** There was, of course, no way in which such varia-

[72] See *In Cael.* 504.17ff., where Simplicius gives an extended quotation from Sosigenes.

[73] *In Cael.* 504.17ff., 20ff., 22ff. As with so many other matters relating to the interpretation of Eudoxus' theory, we are in no position to say with any confidence what his view on the difficulty may have been. Indeed, the very nature of his theory is disputed, for although the evidence provided by Aristotle and Simplicius deals purely with its mathematical characteristics and both sources are silent on the question of Eudoxus' views on the underlying physical realities, the opinion that Eudoxus was quite unconcerned with the latter question is based on no more than that argument from silence. Against what used to be the prevailing scholarly view, Wright 1973–74 has recently argued that some of the mathematical complexities of Eudoxus' theory can only be understood if he had in mind also the physical realities that correspond to the mathematical spheres he postulated. So far as Autolycus goes, neither of his extant treatises, *On the Moving Sphere* and *On Risings and Settings* , deals with the problem of the variations in the distances of the planets, sun, and moon, a problem first successfully resolved by the eccentric-epicycle model elaborated by Apollonius: this belongs to the late third century B.C. , even though features of the model go back to the fourth. The notion of the circum-solar (and therefore epicyclic) movement of the lower planets, Venus and Mercury, is often ascribed to Heraclides of Pontus, though the evidence for this is weak (see Neugebauer 1975, vol. 3, pp. 694ff.). More definitely, some of the Pythagoreans, as we have seen, are reported to have adopted a non-geocentric theory, and Polemarchus, a contemporary of Eudoxus, appears to know, since he rejects, eccentricity (see next note).

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tions in the distance of a given planet could be accommodated within a theory based on combinations of concentric spheres, with the earth at the centre of the system. Yet it appears that Polemarchus of Cyzicus, for one, recognised the difficulty but, not being prepared to sacrifice the assumption that the earth is at the centre, chose deliberately to ignore it, representing

the variation in distance as "imperceptible."**[74]**

Better still than such instances where we depend on secondary reports, we have some fairly explicit texts in Ptolemy that both show that he is aware of some of the problems and give some insight into how he hoped to overcome them. In *Syntaxis* book 9 chapter 2, he sets out the main difficulties that any account of the movement of the planets faces. He appeals to these difficulties as his excuse for allowing himself certain moves or devices. One that he mentions is that he will have to make certain primary assumptions that "do not stem from any readily apparent principle," though they are assumptions arrived at from continuous trial and application.**[75]** His introduction of the equant, or centre of uniform motion, distinct from both the centre of the earth and the centre of the deferent circle, would be one example of such an assumption, though he does not specify it as such here.

The first device he refers to is even more interesting for our concerns. This is the use of certain assumptions he describes as "paradoxical" or "counter to reason,"

παρὰ τὸν λόγον

, and here he does ex-

emplify: "as when we carry out our proofs—to make them more easy to follow—as if the circles described by the movements in the spheres are *bare* [widthless] circles, and as if they are all in the same plane, namely, that of the ecliptic."**[76]** Now, he recognises both that the circles are not, in fact, widthless and that they are not in the plane of the ecliptic. Yet for the sake of his first analysis of planetary motion Ptolemy isolates their longitudinal movements (their motion along the ecliptic) and focuses on them to the exclusion of any consideration of their movements in latitude (that is, north and south of the ecliptic). That achieves a formidable simplification of the problems. Yet it is one for which Ptolemy has a twofold justification. First, as he tells us in 9.2 when commenting on such procedures in general, there can be no objection to them where no appreciable difference, no

ἀξιόλογος διαφορά

, results.**[77]** Although he has no worked-out, indeed no explicit, theory of levels of significance, he is clearly exercising his judgement on whether the differences he is discounting are important, and the *claim* is that those arising from his "paradoxical" assumptions are negligible.

Secondly, and more importantly, even though he discounts movements in latitude in his initial discussion of the models of the planets in book 9, he returns to that very problem in book 13, where he modifies the models in an attempt to account for the observed deviations from the ecliptic. There is no question, then, of his simply *ignoring* the difficulties presented by movement in latitude or of his forgetting that he introduced the simplification in book 9. On the contrary, it is clear that what he borrows for the sake of argument there he pays back in full in book 13.**[78]**

[77] Ptolemy *Syntaxis* 9.2 (H) 2.212.10; cf. above, Chap. 5 n. 203, citing other texts from both astronomy and harmonics in which he appeals to a similar principle.

[78] *Syntaxis* 13.1–5 (H) 2.524.6ff., deals with the problems of the latitudinal movements of the planets. In this context Ptolemy justifies the compli-cated nature of the devices he employs by claiming that they are the simplest possible and by arguing that "simplicity" in regard to heavenly things should not be assessed by the criteria drawn from our ordinary experience of simplicity. Rather, their simplicity should be judged "from the unchangeableness of the natures in heaven itself and of their movements. For thus all would appear simple, and more so than those things that seem so with us, since it is unthinkable that there is any labour or difficulty in their revolutions"; *Syntaxis* 13.2 (H) 2.532.12ff., 534.1–6. Reverting to the issue mentioned in 9.2, he claims at 13.1 (H) 2.525.3ff. that the devices he introduces to deal with the latitudinal movements make no appreciable difference to longitudinal positions.

Parallax Cases

The issue of the negligibility or otherwise of what is discounted takes us to some more complicated cases, such as the various assumptions made by different astronomers in different contexts about parallax. We can distinguish between three main types of case. First, there is the assumption that in relation to the sphere of the fixed stars, the earth may be treated as a point: it is of negligible size, and so it does not matter, in this context, that an observer is not at the centre of the earth, but on its surface, at some distance from that centre.**[79]** This assumption is set out in Euclid's *Phaenomena* , for instance, and in the elementary textbook of Cleomedes.**[80]** It also figures in *Syntaxis* book 1 chapter 6, where, moreover, Ptolemy offers a particularly clear statement of the grounds to justify it: the fact that the configurations of the constellations remain unchanged from whatever point on the earth they are observed *indicates* the very great distance of the stars.**[81]**

A second and far more controversial assumption is that made in

[79] Similarly, from an early stage, neither the existence of mountains and valleys nor the appearance that the setting or rising sun presents of being cut off by a straight line at the horizon was allowed to count against the conclusion that the earth is generally spherical in shape: see, for example, Aristotle *Cael.* 294a1ff., *Mete.* 1.3.340b33ff., and cf. Theon 124.7ff.

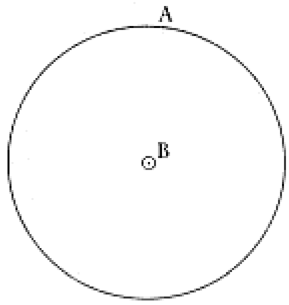
[80] Euclid *Phaenomena* Proposition 1, 10.12ff., Cleomedes 1.11.102.23ff., 106.9ff.; cf. Theon 120.3ff., Proclus *Hyp.* 2.26.26ff., 28.13f., and already in Diocles *On Burning Mirrors* paras. 18ff., Toomer 1976, p. 38 and commentary p. 145.

Aristarchus' heliocentric theory, as reported by Archimedes, namely, that not just the earth but the circle in which the earth moves around the sun is as a point in relation to the sphere of the fixed stars (see Figure 2).**[82]** Archimedes' own comment is that that is, strictly speaking, impossible, since a point has zero magnitude and the fixed stars would then be at infinite distance**[83]** (a similar point applies, of course, to the first type of parallax case as well). What Aristarchus needs is not that the stars be infinitely, only that they be indefinitely, far away.

The interesting feature is that he evidently incorporated this *into his assumptions* , in part in order to meet a possible objection to heliocentricity. If the earth moves in a circle around the sun (rather than the sun around the earth) there should be, one might think, observable differences in the shapes of the constellations as viewed from different points in the earth's orbit—from the points representing the spring and autumn equinoxes, for instance, at opposite ends of the same diameter of the orbit. Yet no such variation was observed; indeed, stellar parallax was not confirmed until well into the nineteenth century, with the work of Bessel and others around 1835. Aristarchus seems to have appreciated that this otherwise very damaging objection to heliocentricity was no objection at all *provided* that the stars are sufficiently far away. If the diameter of the earth's orbit around the sun is negligible in comparison to the diameter of the sphere of the fixed stars, then you would not expect *observable* variations in the relative positions of the stars, certainly not within the limits of ancient techniques of observation. Unlike Ptolemy's discussion of the size of the earth in *Syntaxis* 1.6, the assumption in the form adopted by Aristarchus was not itself justified by reference to independently observable phenomena; there was no way in which it could be. Rather, this reveals precisely what has to be accepted *among the assumptions* in order for an apparent objection from the side of the phenomena *not* to be the objection it seems. No doubt Aristarchus could have argued that the

[82] Archimedes *Aren.* 1.4 (HS) 2.218.7ff.

[83] *Aren.* 1.6 (HS) 2.218.18ff.



Case 1: Ptolemy *Syntaxis* 1.6: circle A = fixed stars;
circle B = the earth.
Case 2: Aristarchus' heliocentric theory: circle A = fixed stars;
circle B = circle in which the earth orbits the sun.
Case 3: Aristarchus' *On the Sizes and Distances:* circle A = moon's orbit round earth;
circle B = the earth.

Figure 2
Three cases of discounted parallax. In each case circle B is treated as a point
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inability to confirm an assumption directly does not make it untrue—and Copernicus would have said the same.**[84]**

The third type of case again comes from Aristarchus, this time from the extant treatise *On the Sizes and Distances of the Sun and Moon* . The second hypothesis set out there is that the earth is as a point, not in relation to the sphere of the fixed stars, but in relation to the *moon's orbit* .**[85]** In this form the assumption involves discounting *lunar* paral-

[84] Cf. Copernicus *De revolutionibus orbium coelestium* (*Rev.*) 1.6. Already those Pythagoreans who treated the earth as one of the planets (see above at n. 12) had argued, according to Aristotle, that there is no difficulty in supposing that the phenomena would be the same as they would be if the earth were at the centre—even though they denied that. "For there is nothing to show, even on the current view, that we are distant half the earth's diameter [viz. from the centre]," Aristotle *Cael.* 293b25ff., 29f.

[85] Aristarchus Hypothesis 2 (352.5f. Heath).

lax—as if the position of the observer on the surface of the earth makes no difference to observations of the moon. Yet of course it does. The contrast with the careful and complex discussions in the *Syntaxis* where Ptolemy attempts to determine the allowance that has to be made for lunar parallax**[86]** is striking and points up the difficulty that the second hypothesis in Aristarchus' treatise presents, indeed, its complete unacceptability *if* we are concerned with trying to establish the *actual* size of the moon and its *actual* distance from the earth.

Yet we should not be misled by one possible way of taking the title of Aristarchus' treatise (*On the Sizes and Distances* . . .) into thinking that that *was* his aim. Both the hypotheses and the results militate

[86] See especially Ptolemy *Syntaxis* 5.11–12, 18–19 (H) 1.401.2ff., 442, 444.2ff.: at 5.11 (H) 1.401.6ff., Ptolemy begins by recalling from 4.1 (H) 1.266.1ff. that the earth does not bear the ratio of a point to the distance of the moon's sphere. With Ptolemy's discussions of lunar parallax we may compare Toomer's proposed reconstruction of the procedures Hipparchus used to determine the distances of the sun and moon (Toomer 1974–75). According to this, Hipparchus took first a minimum, then a maximum, figure for solar parallax, in order to arrive at upper and lower limits for the distance of the moon, expressed in terms of earth radii. First he assumed solar parallax to be zero: using differences in the magnitude of a solar eclipse observed near the Hellespont and at Alexandria, he computed a minimum distance for the moon (71 earth radii). Then Hipparchus took a maximum figure for solar parallax (7') and computed from this the sun's minimum distance and the corresponding maximum distance for the moon (67 1/3 radii in the mean: here Toomer draws on and develops an analysis put forward by Swerdlow 1969). As Toomer emphasised, 1974–75, p. 140, if this reconstruction is correct, one striking feature of Hipparchus' workings would be "the complete honesty with which [he] reveals his discrepant results," namely, that the "maximum distance" obtained by the second method turns out to be *smaller* than the "minimum distance" obtained by the first. There are, as Toomer also stressed, considerable difficulties in interpreting the meagre and at points conflicting reports in Ptolemy and Pappus on which this reconstruction is based. Both sources, however, agree that Hipparchus took a lower limit for solar parallax of zero. As Ptolemy puts it, *Syntaxis* 5.11 (H) 1.402.19f., "at one time he assumes that the sun has no perceptible parallax, at another that it has a sufficiently large parallax [viz. to be observed]," and as Pappus reports in his *Commentary on Books 5 and 6 of the Syntaxis* 67.21ff. Rome: "So Hipparchus . . . assumed in the first book of *On Sizes and Distances* that the earth has the ratio of a point and centre to the sun (i.e., to the sun's sphere)."

against such a view. First, his results all take the form of ratios or proportions, giving upper and lower limits for the relative sizes and distances on the basis of the assumptions as set out; no absolute figures are presented.**[87]** Then, the hypotheses include several that Aristarchus undoubtedly knew to be well wide of the mark. That appears to be the most likely explanation of the notorious sixth hypothesis, that the moon subtends an angle of 2° to the eye;**[88]** where 1/2° was the usual ancient approximation and is indeed the figure we can attribute to Aristarchus on the basis of a report in Archimedes.**[89]** Again, the fourth hypothesis simply assumes, with no attempt at justification, that the moon is at 87° to the sun when it appears to be halved.**[90]** Again, the

[87] For example, Aristarchus Proposition 7 (376.2ff. Heath) ("The distance of the sun from the earth is greater than 18 times, but less than 20 times, the distance of the moon from the earth") and Proposition 18 (410.12ff. Heath) ("The ratio of the earth to the moon is greater than 1259712 to 79507, but less than 216000 to 6859").

[88] Aristarchus Hypothesis 6 (352.14f. Heath) ("That the moon subtends one-fifteenth part of a sign of the zodiac"). The interpretation of this hypothesis has been much debated: see for example Heath 1913, pp. 311ff.; Wasserstein 1962; Neugebauer 1972/1983, p. 366, 1975, vol. 2, pp. 634ff., who concludes, p. 643, "The fact that Aristarchus does not make the obvious transition from the diameters of sun and moon to the distances, measured in earth radii, supports our conclusion that [the Sixth Hypothesis] is not to be taken as a valid observational datum. . . . I think this analysis leads to the conclusion that Aristarchus' treatise on the sizes and distances is a purely mathematical exercise which has as little to do with practical astronomy as Archimedes' 'Sandreckoner' in which he demonstrates the capability of mathematics of giving numerically definite estimates even for such questions as the ratio of the volume of the universe to the volume of a grain of sand."

[89] Archimedes *Aren.* 1.10 (HS) 2.222.6ff. reports that Aristarchus "discovered that the sun's apparent diameter is 1/720th part of the zodiac circle," and the equivalence of the apparent diameters of the sun and moon is shown in Aristarchus' treatise in Proposition 8 (382.1ff. Heath) and indeed assumed from the beginning (cf. Neugebauer 1975, vol. 2, p. 635). After reporting Aristarchus' value for the diameter of the sun, Archimedes himself goes on to describe how he used a dioptra to obtain upper and lower limits for this, namely, 1/164th and 1/200th of a right angle, that is, between under 33¢ and 27': see Shapiro 1975.

[90] In Hypothesis 4 (352.10f. Heath) this is stated in the form: "when the moon appears to us halved, its distance from the sun is then less than a quadrant by 1/30th of a quadrant."

fifth takes it that the breadth of the earth's shadow, viz., at the moon, is two moons.**[91]** Moreover, the second hypothesis itself not only discounts lunar parallax but takes the moon to move in a simple circle with the earth as centre—and no serious Greek astronomer had thought *that* since before Eudoxus.

Such a set of hypotheses would, of course, be an unmitigated disaster in any attempt to arrive at concrete determinations of the actual sizes and distances of the moon and sun. What Aristarchus is doing, rather, is exploring the geometry of the problems. Given certain assumptions—and it will not matter, for the sake of the geometry, that some of the values are a little, and others wildly, inaccurate—what follows? The study is certainly *relevant to* astronomy, in particular because it shows *how* one *could* obtain *actual* solutions to the astronomical parameters, that is, it offers one set of answers to the question of the premises, or data, needed in order to arrive at such solutions. Yet it remains itself essentially a study of the geometry of the problems.**[92]**

The Aims and Assumptions of Greek Astronomers

As this last example shows, certain types of simplifying assumption involve not so much discounting a value that can—with greater or less justification—be deemed to be negligible, as a veritable *mutation* of the problem. Once certain of the known empirical data are suspended, the study becomes one of pure geometry and does not offer to solve, though it remains relevant to, the astronomical problems themselves. Now, it is just such a shift that Duhem and his followers saw as typical of the dominant strand in Greek astronomy: a lack of concern with the physics of the problems in favour of a preoccupation with the mathematics, the construction of models that are purely calculating devices with nothing to do with any underlying physical realities.**[93]** It is

[91] Hypothesis 5 (352.13 Heath).

[92] Cf. Neugebauer 1975, vol. 2, p. 643, quoted above, n. 88.

[93] In his 1908, e.g., pp. 120, 281, 284 especially, Duhem drew a fundamental contrast between two views on astronomical hypotheses. On the one hand, they might be treated as "simple mathematical fictions," "pure concep-tions," where there is no question of their being "true" or even "probable," where "true" is glossed as "in conformity with the nature of things." On the other, they might also be held to describe "concrete bodies" and "movements that are actually accomplished." Duhem is in no doubt that the former represents the position of the major Greek astronomers and commentators: cf. Wasserstein 1962, p. 54, and contrast G. E. R. Lloyd 1978b.

indeed undeniable that there are instances (Aristarchus' treatise is one) where the problems are treated, at least for the time, as problems of geometry. There is certainly a tradition of the investigation of the mathematics relevant and useful to astronomy that exists side by side with astronomy itself—a tradition that goes back to Autolycus' work *On the Moving Sphere* .**[94]** But that cannot be said to vindicate the line of interpretation that Duhem advocated. What that line of interpretation itself discounts, or at least seriously underestimates, is an equally undeniable concern with more than just the mathematics of the problems in much—indeed, in my opinion, most—ancient Greek astronomy.

In what are admittedly complex issues,**[95]** the chief objection to Duhem can most easily be illustrated in relation to Ptolemy himself.

[94] Autolycus is represented as active around 330 B.C. : see Mogenet 1950, pp. 5ff.; Neugebauer 1975, vol. 2, pp. 573, 750f.; Aujac 1979, pp. 8ff. On Autolycus and the tradition he represents, see also I. Mueller 1980, pp. 106ff.

[95] The major difficulty we face is that for several important astronomers, such as Apollonius, we have no direct evidence, and for others, such as Hipparchus, only secondary reports of doubtful reliability. Thus Theon 188.15ff. does not—pace Duhem 1908, pp. 119f.; cf. G. E. R. Lloyd 1978b, pp. 217ff.—characterise Hipparchus as indifferent to physics. On the contrary, he represents him as adopting the epicyclic hypothesis in preference to the eccentric one not for purely mathematical reasons, but on general and cosmological grounds: "it is more plausible that all the heavenly bodies should lie symmetrically with regard to the centre of the universe and be joined together similarly." (Moreover, Theon is not only a naive realist himself, but represents Greek astronomy as a whole as founded on the study of nature, contrasting it with Babylonian, Chaldaean, and Egyptian astronomy in just this regard, 177.20ff.). While the positions of Eudoxus and Callippus on this issue are a matter of conjecture (see above, n. 73), it is clear that Aristotle demanded a physically unified system in *Metaph.L* 8.1073b38ff. As for our single most important and most comprehensive source, Ptolemy, both the *Planetary Hypotheses* (*Plan.Hyp* .) and the *Syntaxis* make it abundantly clear, in my view, that his strategic intention was to provide not just a mathematical, but also a physical account of the phenomena.

First, the treatise known as the *Planetary Hypotheses* provides incontestable evidence that he aimed for a physical account of astronomical phenomena. There he discusses the nature and relations of the spheres on which the

heavenly bodies move. The circles that govern the movements of the planets, sun, and moon are conceived as strips of spheres.**[96]** He even engages with the problems of celestial dynamics, that is, with what makes the heavenly bodies move or the forces at work, when he suggests that we should suppose that each of the planets possesses its own vital force: they move because they are alive.**[97]**

But the case does not rest with that work. Within the *Syntaxis* itself, the very rejection of heliocentricity or, rather, the rejection of the ascription of any movement to the earth shows that, for all the concentration on the mathematics of the problems in that treatise, the whole discussion is set firmly in the framework of certain *physical* assumptions.**[98]** There can be no question of the earth moving, in Ptolemy's view, primarily because of the (apparent) absence of the expected physical effects of its movement on the earth's surface. If the earth rotated, for instance, you would expect the violence of that motion to have visible effects around us. Clouds, or missiles travelling through the air, could, he says, never move eastwards, for they would always be anticipated by the motion of the earth itself.**[99]**

[96] Ptolemy *Plan.Hyp.* 2.4 (H) 113.12ff., and 2.6 (H) 117.8ff. especially.

[97] See *Plan.Hyp.* 2.5 (H) 116.20ff., and 2.7 (H) 119.18ff.

[99] See *Syntaxis* 1.7 (H) 1.24.14ff. The defence against this objection, that the air moves with the earth, is itself countered by Ptolemy at 1.25.15ff. with the argument that composite objects in the air would still always seem to beleft behind by the movement of earth and air together, while if those objects too were carried round "united" with the air, they would never appear to have any independent eastward or westward motion. Earlier in the same chapter, 1.21.14ff., Ptolemy had invoked the Aristotelian doctrine that heavy objects naturally move towards, and rest at, the centre. To the argument from the violence of the supposed rotation of the earth, Copernicus *Rev.* 1.8 countered by referring to the difficulties involved in the incredibly swift motion that, on the Ptolemaic view, the heavens themselves were supposed to make in their diurnal revolution. But for Ptolemy that merely reinforced the belief in the exceptional nature of the substance of which the heavens were made.

To return to the chief point with which we are concerned here, namely, the nature of and constraints on the idealisations that Greek astronomers allowed themselves. Ptolemy, at least, certainly does not permit himself any general escape clause. In every or almost every case where he introduces an idealisation or a simplification, he has to exercise his judgement concerning the magnitude and character of the discounting. So far as his primary assumptions go, he claimed, as we saw, that he adopts something

"paradoxical" only when the consequences fall within the limits of negligible difference. Elsewhere he often justifies the approximations he makes on the grounds of the specific problems encountered in securing reliable data.**[100]** Although we may often question his judgement and may sometimes even suspect his *bona fides* , he does not proceed as if he could allow himself *any* arbitrary adjustment he liked—that is, what *he* could cheerfully agree to be such—an adjustment that would have the effect of turning his discussion into a purely hypothetical, mathematical exercise. The constraints on that discussion come both from the physics of the situation (on the issue of the movement of the earth) and from the astronomical data as secured, as well as may be, by observation.

There is, however, one notable, indeed notorious, instance that looks, and to some extent is, an exception to what is still the general rule. This is the famous case of the discrepancy between what his theory predicts and what is observed on the question of the angular diameter of the moon. It follows from Ptolemy's lunar model that the maximum distance of the moon from the earth is nearly twice the

[100] See above, Chap. 5 at nn. 84–91.

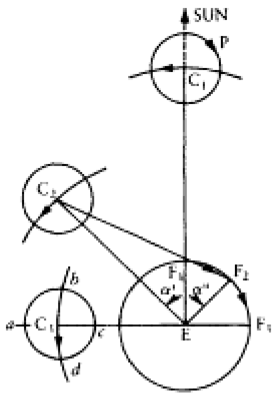

Figure 3
Ptolemy's model to explain the moon's second anomaly.
The moon's epicycle, centre C, moves round a centre (F)
which itself describes a circle round the earth (E). F moves
round E with the same angular velocity as, but in the contrary
sense to, the movement of C round E. At position (1) (syzygy)
the model is equivalent to a simple epicycle model. At
position (3) (quadrature), when the moon is at apogee or perigree
on the epicycle (that is, at *a* or at *c*) the model is again equivalent
to a simple epicycle model so far as the moon's angular
distance from the sun in concerned. But at position (3),
when the moon is at *b* or *d* , midway between apogee and
perigee on the epicycle, the effect of the new model is to

increase the apparent diameter of the epicycle.
Derived from G. E. R. Lloyd, *Greek Science after Aristotle* , fig. 27.

minimum distance (see Figure 3).**[101]** The angular diameter of the moon should, in turn, vary roughly by a similar amount, a factor of 2. Yet it patently does not. Nor does the evidence in the *Syntaxis* suggest that Ptolemy thought that it did. It is true that he gives specific values only for the maximum and minimum apparent diameter at syzygy (not for the maximum at quadrature),**[102]** but those he offers are of the right

[101] Taking the distance from the earth (the centre of the ecliptic) and the centre of the moon's epicycle (R = EC, in the diagram) as usual as 60 parts, Ptolemy estimated the radius of the epicycle ($r = C_1$ P in the diagram) as 5; 15p expressed sexagesimally (i.e., 5 15/60ths) and the eccentricity (e = EF in the diagram) as 10; 19p, *Syntaxis* 5.4 (H) 1.366.15ff. Maximum geocentric distance, at syzygy, i.e., R + r , will then be 65; 15p. Minimum geocentric distance, at quadrature, i.e., R − 2e − r , will be 34; 7p, only slightly over half the value for the maximum. Cf. Neugebauer 1952/1957, pp. 194ff., 1975, vol. 1, pp. 86ff., cf. Pedersen 1974, pp. 192ff.

[102] The modification that Ptolemy introduced to Hipparchus' lunar model was to suppose that the centre of the deferent circle (F in the diagram) itself moves in a circle round the earth (E) at the same angular velocity as, but in thecontrary sense to, the movement of the centre of the epicycle (C) round the earth: angle a = angle a in the diagram. The effect of this modification on the geocentric distance of the moon, and so on its apparent diameter, is at a maximum at quadrature, but decreases to zero at syzygy (conjunction or opposition): in the latter case the difference between apogee and perigee corresponds simply to the epicycle's diameter.

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order of magnitude, a minimum of 31' 20" and a maximum at perigree at syzygy of 35' 20".**[103]** Moreover, and to complicate matters further, the *Planetary Hypotheses* tackles the problem of how the spheres of the heavenly bodies nest into one another, where it is assumed that the maximum distance of one body corresponds to the minimum distance of the one above it, and there Ptolemy clearly accepts the geometrical consequences of his epicycle-eccentric model in the *Syntaxis* as correct.**[104]** So far from being embarrassed by those consequences, he takes them as the basis of his calculations of the absolute distances of the moon and other bodies.

Here, then, we have a major discrepancy between the theory and the data of observation, and Ptolemy's lack of embarrassment just increases ours, since it looks as if he has simply discarded part of the phenomena quite

arbitrarily. That he *has* discarded part of his data is clear. That it is quite arbitrary is more debatable. We have to recall that the theory worked extremely well as a theory of the longitudinal positions of the moon, where it represented a quite marked improvement on Hipparchus' lunar model, which itself gave tolerably serviceable results.**[105]** As regards the lack of appreciable variation in the apparent size of the moon, how—without recourse to desperate expedients—Ptolemy thought he could get round the difficulty we do not know.**[106]** But he may not have despaired—presumably he did not despair—of

[103] See *Syntaxis* 5.14 (H) 1.421.3ff., 6.5 (H) 1.479.14ff. In his comments on the hypotheses in Aristarchus' *On the Sizes and Distances* , Pappus attributes the same figures to Ptolemy, apparently as if they were his definitive values: *Collectio* 6.37.71 (2.556.17ff. Hultsch).

[104] See *Plan.Hyp.* 1 part 2 chap. 3, Goldstein 1967, pp. 7f., and cf. *Plan.Hyp.* 2.16 (138.14ff. H.).

[105] On the superiority of Ptolemy's final lunar model, in *Syntaxis* 5.5 (H) 1.367.3ff., to its predecessors, see Petersen 1969 and Gingerich 1980, pp. 256ff.

[106] Thus there is no suggestion that Ptolemy wavered in his belief in the constancy of the radius of the moon's epicycle, though this possibility has been canvassed in the case of Hipparchus; see Neugebauer 1975, vol. 1, pp. 315f.

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some explanation being possible. In his view, we may assume, the difficulty presented by this phenomenon was not great enough to justify his abandoning the model as a whole.

As we have noted before, until such time as a superior model is available, any scientist would be justified in continuing to maintain, in the face of *prima facie* counter-evidence, a theory that had shown its ability to account for at least part of the phenomena, though every scientist *should* in principle, to be sure, be *especially* self-critical on the question of when the strength of the counter-evidence is such as to make a new model imperative.**[107]** Yet the price Ptolemy has paid in this case is clear: the elision of part of the data is here no mere temporary

[107] This raises, of course, one of the most hotly contested topics in recent philosophy of science, namely, the rationality of different responses to anomalies or counter-examples in scientific research programmes. The

issues have been debated in connection especially with Kuhn's ideas on the generation of "crisis" in normal, puzzle-solving, science, and with those of Lakatos on the relation between the "hard core" of a scientific research programme ("irrefutable" by the methodological decision of its proponents) and its "protective belt" of auxiliary hypotheses (where anomalies may abound and the programme still remain "progressive"—defined in terms of increasing content) and where Lakatos distinguishes between *different* types of ad hoc procedure, including the strategies he dubbed "monster-barring," "exception-barring," and "monster-adjustment" (this last defined as turning a counter-example, in the light of a new theory, into an example). See most notably Kuhn 1962/1970, and 1977; Lakatos 1976, 1978a, e.g., p. 48, p. 63 n. 3; Laudan 1977, 1981a; Newton-Smith 1981; and Hacking 1983. Ptolemy's astronomy as a whole, which has been much discussed in relation to Copernicus especially (Kuhn 1957; Lakatos 1978a, chap. 4, pp. 168ff.), represents, from an ancient perspective, the most elaborate and fully developed research programme in ancient science—and it was one that was to prove extremely durable. The example of the variation in the angular diameter of the moon shows that, on occasion, Ptolemy, confronted with an anomaly, offered no adjustment but simply passed it by in silence. When the general problem of the apparent complexity of his devices is explicitly raised *as a problem* , in *Syntaxis* 13.2 (H) 2.532.12ff., his response, as we have seen, n. 78, is to *claim* that he has attempted to use, so far as possible, the simplest hypotheses, but then also to argue that "simplicity" in regard to heavenly things should not be judged by our ordinary criteria of simplicity. At this, the point where the question of the viability of the system as a whole comes, perhaps, closest to the surface, there is an uncomfortable tension between simplicity invoked as a *criterion* to judge between theories, and the *axiomatic* assertion that the movements of the heavenly bodies are simple, however complicated they may appear to us.

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simplification for the sake of argument, but represents a limitation on the viability of the model itself.

Conclusions for the Exact Sciences

Our survey, to date, of some of the main types of simplification and idealisation in Greek science has been drastically selective, but some points have, I hope, emerged sufficiently clearly. The move to discount some of the phenomena in question is associated with some of the most notable successes of Greek science. On occasion, to one looking back with the benefits of hindsight, it seems as if the problem in ancient science is too little abstraction from the complexities of the phenomenal situation, rather than too much, though there was—there is—no way of telling in advance when this may be the case. Some attention, at least, is paid by Ptolemy, for one, to the conditions under which simplification is possible, though he provides no exact rules, only rough-and-ready practical guidelines, with his

appeal to a vague, certainly unquantified, notion of "negligible difference." That already indicates some awareness of the problem of discounting parts of the phenomena to leave in play only what is readily explicable. While this was often a sensible policy, it could also prove all too facile a manoeuvre, when recalcitrant data that are central to a problem are simply ignored and when there is no question of their being reintroducible, in principle, at a later stage, with the theory remaining intact.

The Life Sciences: Teleology

Thus far we have taken our examples from the exact sciences, but comparable moves can be documented also in other areas of ancient speculative thought with results that must provoke further reflection on the aims and methods of ancient investigators. Teleology offers a general rubric under which we can discuss some especially striking examples from various areas of the life sciences.**[108]** From the time of

[108] On the pre-Platonic background see especially Theiler 1924, Solmsen 1963, Balme 1965; cf. Sorabji 1980a.

Plato, at least, the notion that the world as a whole is well-ordered, the product of divine design, is one of the most powerful motive forces in ancient science, though it remained a far from unanimous, indeed a much contested, view.**[109]** The question we may raise in this regard is whether or how far teleological accounts were secured at a cost of discarding part of the phenomena in a way that is broadly comparable with the elisions we have studied from the exact sciences.

In the first developed statement of a teleological cosmology, in Plato's *Timaeus* —and often thereafter—it is explicity recognised that other factors besides the good have to be taken into account. Plato's Demiurge and the workings of reason are confronted with the factor of necessity, exemplified by the concomitance of material properties, as when the hardness of bone necessary to protect the head is inseparable from a heaviness that weighs it down and makes for insensitivity.**[110]** The human race would have been longer-lived but less intelligent if the head had been covered by a thicker layer of bone: as it is, unable to secure *both* long life *and* intelligence, god sacrifices the former, lesser end to insure for humans, as far as possible, a noble and intelligent, if shorter, life. In Aristotle, too, the final cause is often contrasted with what he too calls necessity—that is, simple, not conditional, necessity**[111]** —again associated with the material properties of things. Some

[109] Teleological explanation was, of course, denied, both before and after Aristotle, by the atomists especially.

[110] Plato *Ti.* 74e ff. On the role of necessity and of what Plato calls the "wandering cause" (*Ti.* 48a7) in the *Timaeus* , see especially A. E. Taylor 1928, pp. 299f.; Vlastos 1939/1965, 1975a; Morrow 1950/1965; Alt 1978.

[111] As we have noted before, Chap. 3 at n. 149, Aristotle distinguishes between several different types or modes of necessity in passages in the Organon, the *Metaphysics* , and the physical and biological works, and the interpretation of his distinctions and the question of their consistency have been the subject of protracted recent debate. Apart from (1) necessity in the sense of the compulsory or the violent, and (2) absolute or unqualified necessity relating to "eternal" things, including both (2a) the eternal heavenly bodies, and (2b) the (timeless) truths of mathematics, two further types of necessity are of particular concern to the natural philosopher. (3) "Conditional" or "hypothetical" necessity is explained in terms of the material conditions necessary for an end to be realised; thus for a house to be built, matter of a particular kind must be available; for an axe to be able to cut, it must be of suitable material. But this isin turn contrasted with (4) the necessary consequences of the natures of things or of their being as they are. While (3), conditional necessity, works *with* the final cause, as the necessary condition for the realisation of some good, (4) is sometimes contrasted with the good. The distinction is drawn at *PA* 642a31ff. in connection with the example of respiration, where (4) is illustrated by the necessary behaviour of the hot substance moving in and out and of the air flowing in. Again, at *PA* 677a11ff., discussing bile, Aristotle contrasts those residues that nature is able to turn to some advantage with those where this is not the case. At *PA* 662b23ff., 663b22ff., for instance, he had pointed out that in the case of horns, surplus earthy matter is made to serve the purposes of self-defence and attack, but bile itself is not "for the sake of something" but arises merely as the *consequence* of other things that *are* for some good (*PA* 677a16ff.). Again at *GA* 5.1.778b10ff., 16ff., when he contrasts what arises merely in the process of generation or formation with what is contained in the essence of an animal and is for the sake of some good, he distinguishes the necessity for an animal to have an eye (where that characteristic belongs to the essence of the animal in question) and the necessity for it to have an eye of a particular kind (the result merely of the natural processes of formation). Cf., for example, *APo.* 94b27ff., *Ph.* 2.8.198b10ff., *GC* 337a34ff., *PA* 1.1.639b21ff., 642a1ff., *Metaph.* 1015a20ff. The issues are controversial, but among the most important recent studies are Balme 1965, 1987; Kullmann 1974a, 1979; Preus 1975; Gotthelf 1976–77; Nussbaum 1978, pp. 59ff.; Sorabji 1980a; Waterlow 1982a; Lennox 1981, 1985; Leszl 1982; Cooper 1982, 1987; Furley 1985.

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things are so because they have to be so, or happen to be—the colour of the

eyes, for instance—not for the sake of some end.**[112]**

Since teleology does not apply without exception in Plato or Aristotle,**[113]** there is no need for failures of the good to be *denied* —though both philosophers will insist that the Demiurge or nature has secured

[113] The limits of teleological explanation are also discussed in Theophrastus' *Metaphysics:* see above, Chap. 3 at nn. 161ff.

the best possible results, within the constraints of necessity.**[114]** Failures need not simply be elided, since they can be laid at the door of necessity or the recalcitrance of matter, even though for Plato, and to some extent for Aristotle, that risks amounting to a concession that they are, in that respect at least, *beyond* explanation, since a *proper* explanation, by definition, will be in terms of the good.**[115]**

Moreover, the *normative* role of the concept of nature in Aristotle, in the zoological treatises especially, deserves remarking. His official and explicit statement, many times repeated, is that nature corresponds to what happens always or for the most part.**[116]** In practice, however, "natural" is sometimes reserved not for what happens usually, but for what is quite exceptional.**[117]** This is the case, for instance, where we are told that in humans alone the natural parts are fully "according to nature": in humans alone the "upper" part is directed to

[114] See, e.g., Plato *Ti.* 30a, Aristotle *Ph.* 259a10ff., 260b21f., *GC* 336b27f., among a very large number of other texts. Similar themes often recur, of course, in both later philosophy and science, as, most notably, in the Stoic doctrine of providence and in Proclus, both in his commentaries on Plato (e.g., *In Ti.* 1.370.13ff.) and in *Hyp.* (e.g., 1.4.20ff.). Cf. also on Galen, below at nn. 126ff.

[116] See especially *Ph.* 2.8.198b10ff., 34ff., 199b23ff., on which see most recently Furley 1985.

the "upper" part of the universe, and the right side is *most* rightsided.**[118]** Here what Aristotle describes as "natural" is what he deems to be best, where he uses the human species as the norm by which the rest of the

animal kingdom is to be judged. By that standard all other animals fall short. Yet the ideal they fall short of is still referred to as what is "according to nature" or "natural," despite the fact that this picks out not what happens always nor even for the most part, but what is true of humans alone.

Many of the parts and functions of the lower animals are thus evaluated from the point of view of those of higher species, especially of the supreme species, humans. The heuristic value of this idea is clear. It enables Aristotle to recognise, for example, that in the so-called bloodless animals there is an analogous fluid that performs the same functions as blood,**[119]** or, again, that there are analogues to the heart, in his view the control centre of the vital functions.**[120]** Yet, equally clearly, he is led to make some very dubious value judgements. He speaks repeatedly of the parts of certain animals, or of whole species, as *deformed* or *maimed* , using the very same terms,**[121]** such as

ἀνάπηρος, πεπηρωμένος,

κολοβός

, that are used of deformed individual specimens (as it might be an octopus with a tentacle missing). We can understand the use of such terms in relation to the mole's eyes, for example, which he believes not to function as eyes.**[122]** Yet he also calls the whole genus of

[118] See especially *PA* 656a10ff., *IA* 706a16ff., 20ff., b9f., *HA* 494a26ff., 33ff.

[119] See especially *HA* 489a20ff., *PA* 645b8ff., 648a1ff., 19ff., 678a8f., *GA* 728a20f.

[120] What is analogous to the heart exists in lower groups of animals not only as the receptacle for what is analogous to blood (e.g., *PA* 665b11ff.), but also as the centre of perception, imagination, and locomotion in those animals that have them and indeed of life in general (e.g., *Juv.* 469b3ff., *PA* 647a30f., 678b1ff., *De Motu Animalium* 703a14ff., *GA* 735a22ff., 738b16f., 741b15ff., 742b35ff., 781a20ff.).

[121] See, for example, *PA* 684a32ff. (on lobsters), *IA* 714a6ff. (on flatfish), *PA* 695b2ff. (on fish in general), and other texts discussed in G. E. R. Lloyd 1983a, pp. 40f.

[122] *HA* 533a2ff., but cf. *HA* 491b27ff. and *De An.* 425a10f. Compare also *PA* 657a22ff. on the seal's "ears."

the

ὀστρακόδερμα

, testacea (including, for example, the snails, mussels, and oysters), "maimed, as it were," in that the way they move is "contrary to nature."**[123]** Again, compared with humans, *all* other creatures are said to be "dwarf-like"—in that they have their "upper" parts, or those near the head, larger than the "lower" ones.**[124]** It is not that Aristotle's teleology leads him straightforwardly to *deny* the phenomena, but he certainly denies that some are (fully) natural, and this tends to downgrade them as the subject matter for the inquiries of the physicist.**[125]**

Similar tendencies are particularly prominent in Galen. The whole of his treatise *On the Use of Parts* and many other extended passages in other works are devoted to establishing and illustrating the thesis that nature does nothing in vain, which he often, indeed generally, construes as not just a general but an exceptionless rule: *every* part has a purpose, and nature is perfect.**[126]** But the tension this thesis sets up can be seen in his anatomy, his physiology, and his pathology.

[123] *IA* 714b8ff., 10ff., 14ff. Cf. also *PA* 683b18ff.: the testacea, having their head downwards, are said to be "upside down," as also are the plants, in that they take in food through their roots (cf. *PA* 686b31ff., *IA* 705b2ff., 706b5ff.).

[124] "Upper" is defined functionally in relation to the intake and distribution of food. See *PA* 686b2ff., 20ff., 689b25ff., 695a8ff. At *IA* 710b12ff. infants are said to be dwarf-like in comparison with adults.

[125] As we have noted before (Chap. 1 n. 160), Aristotle explicitly states at *PA* 645a15ff., 21ff., that all natural things, and especially all animals, have some share of the admirable and the beautiful—and this insures that all are accordingly and to that extent worthy of investigation. Yet the shares are evidently unequal, and while in principle there need be no reason why downgrading in regard to a creature higher in the hierarchy should entail downgrading as a subject of study, it follows from Aristotle's insistence on the physicist's concern with the final cause (and the good) that the extent to which they are manifested in any given subject matter has direct repercussions on how worthwhile the investigation of that subject matter is, at least under that heading. Moreover, in practice, in the zoological treatises, although there are exceptions, the attention Aristotle devotes to the various kinds of animals broadly reflects, even if it is not to be precisely correlated with, his view of their position in the overall hierarchy.

[126] The strong thesis—that there is no possible improvement to the work of nature—appears, for example, in *UP* 1.5 (H) 1.6.18ff., (K) 3.9.4ff., 3.10

(H) 1.177.20ff., (K) 3.242.5ff., 3.16 (H) 1.190.10ff., (K) 3.259.3ff., and 5.5(H) 1.267.12ff., (K) 3.364.17ff. (where the Erasistrateans are rebuked for failing to demonstrate how nature is worthy of praise in detail—by considering each organ in turn). Elsewhere, however, Galen recognises that nature cannot achieve perfection (see below [at n. 139]) and must weigh the balance of advantage (e.g., *UP* 5.4 (H) 1.260.1ff., (K) 3.354.17ff.).

Thus, developing a common Greek idea,**[127]** Galen represents apes as caricatures of human beings.**[128]** Yet, as is well known, he often uses apes —as others had done before him—as the basis of his anatomical descriptions of humans; his account of the muscles, for example, explicity derives from his dissections of apes.**[129]** From the point of view of transferring conclusions to human anatomy, the ape had better be as close to us as possible.**[130]** Yet so far from some of Galen's great admiration for humans extending to the ape, he calls the ape ridiculous: it has a ridiculous soul and so also a ridiculous body.**[131]** Yet why, if nature does nothing in vain, it plays such jokes, is not explained—and similar points apply also, regrettably, to Galen's account of the human female.**[132]**

[127] See, for example, Aristotle *Top.* 117b17ff., cf. Archilochus 81, 83, Semonides 7.71ff., Plato *Hp.Ma.* 289a–b, cf. Vegetti 1983, pp. 59ff.

[128] See, for example, *UP* 1.22 (H) 1.58.18ff., (K) 3.79.18ff., 3.8 (H) 1.152.21ff., (K) 3.208.15ff., 3.16 (H) 1.194.11ff.,(K) 3.264.9ff., 11.2 (H) 2.117.14ff., (K) 3.848.8ff., 15.8 (H) 2.367.15ff., (K) 4.252.5ff., *AA* 4.1 (K) 2.416.3ff.

[129] See, for example, *AA* 4.2 (K) 2.423.5ff., 5.9 (K) 2.526.4ff. and cf. 1.2 (K) 2.222.2ff., 3.5 (K) 2.384.12ff., 6.1 (K) 2.532.5ff., 6.3 (K) 2.548.2ff.

[130] Galen recognises, indeed, that different species of apes resemble humans to different degrees, and he recommends using those that are most like man, with short jaws and small canines: *AA* 1.2 (K) 2.222.5ff., 223.9ff., 6.1 (K) 2.532.5–535.15 (specifying apes with an upright gait, a thumb in the hand, and a temporal muscle like that in humans), *UP* 11.2 (H) 2.114.17ff., (K) 3.844.7ff.

[131] See *UP* 13.11 (H) 2.273.8ff., 23ff., (K) 4.126.1ff., 15ff., in addition to the passages cited above in n. 128.

Elsewhere—in, for example, his account of the blood-vascular system—we can see the heuristic value of the principle that each part serves a purpose, yet his identification of purposes is selective. Thus he infers that blood is transferred from the right to the left side of the heart, in the adult, through pores in the septum separating the ventricles. He does not claim that these pores can be seen, though pits in the septum suggest their beginnings.**[133]** But they are necessary to account for blood in the arterial system.**[134]** However, he knows very well that in the *embryo* there is a direct route for the blood between the two atria, namely, through the foramen ovale, and he also knows that this closes after birth.**[135]***Why* it should close should be a problem, since if it had remained open nature would not have needed the interventricular pores. Yet Galen quite fails to discuss this, merely remarking how marvellous it is that the foramen closes after birth and *asserting* that it would be of no use in the adult.**[136]** It is enormously to his credit that he realises that the communications in the embryo heart differ from those in the child once born, and it is to his credit too that

[133] *Nat.Fac.* 3.15 (H) 3.251.27ff., (K) 2.207.17ff.

[135] See especially *UP* 6.21 (H) 1.371.4ff., (K) 3.510.2ff., 15.6 (H) 2.360.19ff., 361.12ff., (K) 4.242.18ff., 243.18ff., *AA* 12.5 (pp. 118ff. D.). Galen also clearly knows of the ductus arteriosus, the communication, in the foetal heart, between the pulmonary artery and the aorta, e.g., *AA* 12.5 (pp. 118ff. D.), 13.10 (p. 179 D.), cf. (K) 2.828.10ff.

[136] See *UP* 6.21 (H) 1.374.4ff., (K) 3.514.2ff., and 15.6 (H) 2.362.1ff., (K) 4.244.14ff.

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he does not fudge the function of the foramen ovale, of which he provides the first extant description.**[137]** Thus there is no question of his attempting to *deny* that the foramen exists or that it acts *as* a foramen. Nevertheless, it is striking that his thinking is sufficiently compartmentalised for the difficulty I have mentioned *not* to have occurred to him.

Finally, a far more massive elision is involved in his treatment of diseases, where we may recall some of the problems raised in Chapter 1. How, we may ask, does an out-and-out teleologist account for diseases? Here, if anywhere, there is evidence of a failure of the good.**[138]** One argument that was available, and that Galen duly uses, is that nature can only achieve as good results as the material she has to work with will allow.**[139]** Left to herself, Galen says, nature would have made us immortal.**[140]** He also argues that residues, for example, are formed as the necessary by-products of other physiological processes that are essential to secure some good, and, again, that potentially damaging bile is needed to counteract the potentially

damaging phlegmatic residues:**[141]** one thing leads to another. He asserts that when the animal is healthy, there is no danger, but adds that nature foresaw that it would be easy for excessive residues to be purged from the stomach by vomiting—a remedy long used by Greek doctors.**[142]** Nature evidently needs a helping hand; but Galen still fails to confront the question of the break-down of the system in ill-health. What *good* does that do?

[137] Whether Galen himself was the discoverer is not clear and has been doubted principally on the inconclusive grounds that he makes no claims in that direction (see May 1968, vol. 1, p. 331 n. 102, with references to earlier literature).

[139] See, for example, *UP* 3.10 (H) 1.175.3ff., (K) 3.238.11ff., 5.4 (H) 1.260.5ff., (K) 3.355.4ff., 17.1 (H) 2.446.11ff., 19ff., (K) 4.358.14ff., 359.6ff.

[140] *UP* 14.2 (H) 2.285.7ff., (K) 4.143.5ff., cf. also 5.4 (H) 1.260.7ff., 13ff., (K) 3.355.5ff., 11ff.

[141] See *UP* 5.3 (H) 1.255.6ff., 257.4ff., (K) 3.348.4ff., 350.16ff., and 5.4 (H) 1.258.26ff., 259.6ff., 263.20ff., (K) 3.353.7ff., 15ff., 359.17ff., especially.

[142] *UP* 5.4 (H) 1.259.11ff., 262.17ff., 263.1ff., (K) 3.354.3ff., 358.9ff., 18ff.

None, obviously; it is simply the necessary consequence of the materials we are made of. But the problem is that that factor had been appealed to before, to account for how we come to have residues in health. Why those potentially damaging residues should get out of control, why there is a disruption of the *status quo* in the body, is left unexplained, at least unexplained in teleological terms.

The thesis that nature *always* acts for the good can only be sustained by resolutely focusing on some parts of the phenomena to the exclusion of others that had also to be reckoned as part of common knowledge. The failure of the animal kingdom to be humans is not allowed to count as counter-evidence even to the weaker, qualified Aristotelian version of the thesis, for many animals, as degenerate or deformed, are not fully "natural." Nor are evident inconveniences in the anatomy or the physiological processes of humans allowed to tell against the stronger, Galenic position, and no more are diseases. It is not that teleology as such is mistaken in principle. On the contrary, in many areas and on many questions it proved

itself in the ancient world—as it was also to do later—a marvellously powerful heuristic tool. Yet the negative features of its use, as a device for exclusion, for foreclosure, are manifest.

To try to understand this dominant—though, it should be repeated, far from universal—trend in ancient science, it may be helpful to recall our earlier discussions of the extent to which Greek scientists offered accounts that did not merely differ from but directly rivalled and aimed to supersede traditional mythical and religious beliefs and attitudes.**[143]** While many ancient scientists had no intention of incorporating a moral message in their work, many others had and did. Even where that was not the primary motivation of their inquiry, it was sometimes an adjunct to it. Ptolemy not only draws personal comfort from the order revealed by astronomy, he claims (following Plato) that it improves men's characters,**[144]** and that the same is true also of the

[143] Cf. above, Chap. 1, pp. 46ff.

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study of harmonics.**[145]** But the revelation of order is not, of course, by itself bad science. The nub of the question is what kind of order, and how—with what scruples—secured. The double bind on the teleologists was that the greater the potential strength of the moral message concerning the beauty and goodness of nature, the more had to be set aside and either ignored completely or set down to necessity and to the recalcitrance of matter. The most striking examples of the difficulty arise in connection with claims made in the life sciences, but the exact sciences too exemplify the point,**[146]** and even where the good is not at issue, there were hesitations and waverings on the limits of permissible, and those of necessary, idealisations.

[145] See especially *Harm.* 3.4ff. (94.24ff. D.), on which see E. A. Lippmann 1964, chap. 2. The more general idea that harmonics reveals and demonstrates the orderliness and rationality of the works of nature (e.g., *Harm.* 1.2 [5.19ff. D.]) is echoed also by Porphyry *In Harm.* 24.22ff. D. Again, the notion of the moral value of music has the authority of Plato, e.g., *Ti.* 47c–d, cf. 80a–b, *R.* 522a. At *Harm.* 2.31–32, Aristoxenus mentions the view that the study of harmonics can make you a better person, but there expresses his own reservations on the topic.

The Value and Effect of Science in the Ancient World

These case-studies have raised a number of extremely general issues in the interpretation of ancient Greek science, and we may, in conclusion—and at the risk of still further sweeping generalisations—broach as a final topic the question of some of its values and effects. From the point of view of the ancient world it is worth asking what difference science made. The question relates to *their* point of view, since from *ours* parts of the answer, concerning what difference their science made, must be clear. From the Renaissance on, the myths and realities of Greek science have been enormously influential: myths, because the ancients' ideas have often been distorted when invoked on either side of later disputes, whether to be idealised or to be reviled; realities, because not everything that Greek science has been taken to stand for is mere fantasy, in particular not certain key methodological notions, including those of the value of empirical research, of the application of mathematics to the understanding of the physical world, and of an axiomatic deductive system. The repercussions both of those myths and of those realities have been immense, even though it goes without saying that not every idea influential in the rise of modern science has an ancient antecedent, real or mythical—in particular not our intense preoccupation with the possibility that science, by being applied, may provide the key to material progress and prosperity (an idea only modestly represented in the ancient world).**[147]** Moreover, when translated into modern terms and given an institutional framework as a result partly of that preoccupation, what were mere aspirations towards understanding and control in the ancient world have certainly been transformed in the process of their very actualisation.

[147] While the idea of the *past* advance of civilisation from a state of primitivism can be exemplified readily enough in poetry, in history, and in philosophy (the evidence is collected and discussed by Edelstein 1967b and by Dodds 1973 especially), the notion of *future* progress tends to find expression primarily in the context of the spiritual, not the material. Aristotle, for instance, occasionally states that nearly all possible discoveries and knowledge have been secured already (see *Politics* 1264a1ff., *Metaph.* 981b20ff.).

The blunter and in some ways harder question I proposed concerned the ancients themselves. There the inquiry into nature was generally an activity confined to a tiny elite and intelligible to not many more. Natural science, and even mathematics, established only very limited bridgeheads in what passed for moderately general education. Long after the correct explanation of eclipses was available, ordinary soldiers—and some of their generals—were still capable of being frightened by them, as the debacle of the Athenian retreat from Syracuse illustrates.**[148]** Moreover, in that instance, those ordinary soldiers were nevertheless able, at least according to the

story in Plutarch, to win their freedom, in some cases, by reciting passages from Euripides.**[149]**

Even among the literate elite themselves, the gap between those who were capable of independent research and those who merely knew something about it was very great, as the immensely learned, but at points quite uncritical and confused, Pliny illustrates.**[150]** Introductory or elementary textbooks came to be produced in mathematics, astronomy, and medicine, but in late antiquity this increasingly had the negative effect of defining the outer limits of what there was to know, as much as the positive one of increasing the chance of what was within those limits being preserved.**[151]** Medicine, to be sure, was al-

[148] Thucydides 7.50; cf. Plutarch *Nicias* 23, which claims that while many understood that *solar* eclipses were caused by the moon, they had no explanation for lunar eclipses. Among well-known earlier texts that express some consternation at solar eclipses are Archilochus 74 and Pindar Paean 9.

[149] *Nicias* 29.

[150] Aspects of this in connection with the botanical and pharmacological sections in *HN* are discussed in G. E. R. Lloyd 1983a, part 3, chap. 3, pp. 135ff. Cf. Green 1954; Vegetti 1983, pp. 91ff.

[151] Thus in astronomy the phenomenon of the precession of the equinoxes, which had been discovered by Hipparchus and was the subject of a careful discussion in Ptolemy, was either ignored completely or flatly denied by later writers: see, for example, Proclus *In Ti.* 3.125.15ff., *Hyp.* 5.136.4ff., 7.234.7ff., Philoponus *De opificio mundi* 3.4.117.12ff. While Galen prides himself on his use of proof *more geometrico* , he expresses some diffidence in offering a geometrical demonstration of some optical phenomena in *UP* 10.12ff. (H) 2.93.5ff., (K) 3.812.14ff., putting it in 10.14 (H) 2.109.8ff., (K) 3.835.17ff., that most people would rather suffer anything than have to do geometry, and claiming that he has omitted many proofs that require astronomy,geometry, music, and so on from his works so that they will not be utterly hated by doctors, (H) 2.110.9ff., (K) 3.837.7ff.

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ways of exceptional general interest.**[152]** The other main area that involves the inquiry concerning nature where knowledge and interest extended beyond a small minority was astrology, which must be granted an important positive role in keeping some scientific knowledge alive, since, as we remarked before, some of the same framework of theory underpinned it as underpinned the study of planetary motions in themselves.**[153]**

Much of the otherwise reasonably well-educated or well-read public remained very largely ignorant of advanced natural science. There were particular discoveries—such as that of the vast size of the universe in comparison with the earth—that might have had important repercussions on common assumptions, but they did not, or did not to any great extent, even when they were not totally ignored.**[154]** The day when science could shake the whole foundation of the belief in the privileged place of man in the universe was not yet. The ancients themselves often maintained the belief in some form, and they tended rather to be fortified in it by their scientific studies.**[155]**

[152] Yet the shrinking of medical knowledge can be illustrated in connection with the production of medical encyclopedias in late antiquity. In the mid-fourth century, Oribasius, encouraged as he tells us by the emperor Julian, collected "all that is most important from all the best doctors" in a comprehensive *Medical Collection* in seventy books (see *Coll.Med.Reliq.* 1 praef. 2, *CMG* 6.1.1.4.7f.). But Paul of Aegina, in the seventh century, referring to Oribasius' work in the proem to his own treatise, says that it is too bulky and offers his own shorter compendium: *CMG* 9.1 praef. 4.6ff.

[153] See above, Chap. 1, pp. 45f.

[154] On ancient views of the dimensions of the universe, see Préaux 1968, 1973, pp. 202ff. especially. There were, to be sure, those who denied that the universe is finite, but when, for example, infinite worlds were located beyond *our* heavens, that idea was not necessarily, and not even often, combined with any definite notions of the dimensions of our world: cf. Furley 1981a. One writer who did, however, draw out some of the implications of the minuteness of the earth in comparison with the sphere of the fixed stars is Seneca, for example at *Quaestiones Naturales* 1 praef. 11–13.

[155] This is especially true of zoological and anatomical studies such as those of Aristotle and Galen which we have considered, in which man is treated as the supreme animal.

One general moral was, however, quite widely learned. Natural scientific explanations appeared to have enough success to justify the general claim that natural phenomena have naturalistic explanations. Yet even here bad examples (such as the quite speculative and largely imaginary theories of thunder and lightning) were cited as often as good ones (such as eclipses), and it is notable that those who used this argument in late antiquity were more often philosophers—such as the Epicureans**[156]** —than those who actually engaged extensively in advanced scientific research. Meanwhile, more sinisterly, those successes of science, especially the demonstration of the orderliness of heavenly motions, were also appealed to, as early as

Plato, in order to support a particular view of the moral governance of the cosmos, itself invoked—in Plato's case—as justification for drastic measures of social control directed against atheists and dissidents of every kind.**[157]** More generally, whenever the order revealed in nature could be represented as hierarchical, this provided grist to authoritarian mills.**[158]**

As seen by the average theatre-goer, those who studied nature were figures of fun, in Aristophanes' day, in Plautus'—and in Molière's.**[159]** Natural science was thus assimilated to any other kind of mumbo-jumbo or wonder-work, including to some more traditional modes of

[156] Yet, for the Epicureans, inquiry ceased when *some* explanation was available: see above, Chap. 3 n. 239.

[157] In *Laws* 10, the various types of atheists, who include those who attempted materialist cosmologies based on the denial that soul is prior to body, are made subject to penalties that are set out at 907d ff. The mildest of these—for anyone who does not heed warnings to reform and is convicted of impiety—is imprisonment, but death is prescribed in many cases.

[158] As Aristotle puts it at *Metaph.* 1075a16ff., "everything is ordered together in a way, but not in the same way," and he uses the example of a household and the distinction between slave and free to illustrate how different places are occupied by different kinds of being. Cf. *Politics* 1256b15ff., where plants are said to be for the sake of animals, and the other animals for humans, and the extended argument in *Politics* 1.1–5, especially 1253b14ff., 1254a17ff., maintaining that the distinction between the function of ruling and of being ruled—and so also that between master and slave—is natural.

[159] Aristophanes' targets range over a wide spectrum, including not just the new learning (represented in different ways by Socrates and by Euripides) but also more traditional modes: prophecy and divination are often his butt (e.g., *Pax* 1045ff., *Av.* 961ff.) and temple-medicine is at *Plutus* 665ff.

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special wisdom such as prophecy or divination. Furthermore, although some ancient natural scientists were keen to differentiate themselves from rivals, whether from divination, from philosophy, or from within science itself, others, as we have seen, sought, rather, to associate their activity to moral philosophy (astronomy is good for the soul) or even to religion,**[160]** as when Galen talks about the study of the parts of animals in terms drawn from the mystery religions and speaks of his own book on that subject as a *hymn* to nature and, indeed, superior to ordinary hymns.**[161]** In part this

simply reflects the modalities of the expression of the theoretical motivation of scientific research (which was not the only possible motivation; there were other, practical ones as well, especially in medicine).**[162]** But while to assimilate science to moral

[162] The possible practical applications of the knowledge gained from anatomical dissection, for instance, are emphasised by Galen in *AA* 2.2–3 in texts discussed above, Chap. 3 at nn. 229ff.

philosophy, to the pious worship of nature, even to prophecy, might make it more prestigious, even more comprehensible to a certain audience, this also masked certain differences between science and other modes of wisdom. Anatomical research might be described by Galen as an initiation into the mysteries of nature, but in many respects it was unlike any other initiation: it was harder work, and the results obtained were subject to a different kind of scrutiny and verification. Ptolemy too might hope that studying examples of heavenly order might make you more orderly in everyday life. But, again, that heavenly order was to be revealed only by distinct and rather rigorous methods.

Much of the ancient inquiry concerning nature was formalised common knowledge, and much was fantastic speculation. But some of it was neither, as we can see from such examples as the proofs of the sphericity of the earth, or of Archimedes' principle, or of the role of the valves of the heart, or by such discoveries as that of the precession of the equinoxes, or the nervous system, or the diagnostic value of the pulse. To express an allegiance to the *principles* of engaging in research and of securing a comprehensive and reliable data base, to the need to put theories to the test, to expose and root out unexamined assumptions, to withhold judgement where the evidence was insufficient, to acknowledge your own mistakes and uncertainty—all this was often no more than a matter of paying lip-service to high-sounding ideals. But if this was to bluff (and as we have seen, it often was), it was a bluff that could be called, and we have also seen how, on occasion, it was called, and how the ideals were at least sometimes lived up to and the promises they implied fulfilled.

For all the more or less ill-informed, at the limit actually malicious, confusion of science with some other kinds of wisdom—a confusion to some extent fed by the scientists themselves—it was, as those very scientists were, to judge from their practice, well aware, a wisdom with a difference. It was a wisdom committed to different procedures of discovery and of the justification of belief—even if the full force of those differences was hardly *generally* appreciated, and even if the full demonstration of its potential had to wait until modern times. It was more vulnerable than other modes of wisdom, since in principle it incorporated within itself an invitation to challenge its

results (it gave

hostages to its opponents); at the same time it was more secure, insofar as parts of those challenges could be withstood successfully. It played for higher stakes, and sometimes won, even if not as often as it claimed. The distinction between science and myth, between the new wisdom and the old, was often a fine one, and the failures of ancient science to practise what it preached are frequent; yet what it preached was different from myth, and not *just* more of the same, more myth. The rhetoric of rationality was powerful and cunning rhetoric, yet it was exceptional rhetoric, not so much in that it claimed not to be rhetoric at all (for any rhetoric may aim to conceal itself), as in supplying the wherewithal for its own unmasking—even if some of its exponents did not notice that the mask was still in place. If many of the new wise men were short on delivery, they were long on aspiration, and the aspirations were of a kind that were, in time, to produce extraordinary delivery.

Meanwhile, however, the fact that in its beginnings, science was often explicitly concerned, if sometimes rather naively, with the moral dimension of the activity of science itself reminds us, if we need reminding, that it originated in no merely intellectualist debate. Indeed, its offering an alternative world view, in the widest sense an alternative morality, was central to some of its confrontations with traditional wisdom, though the fact that science may and to some extent must incorporate such values was rather to be lost sight of in the aftermath of the scientific revolution and has only gradually come to be recognised once more in recent times. There is a moral for us today, too, in the point that, again from its very beginnings, we can detect some tension in disciplines that professed that they must give a public account of themselves but that, to a greater or lesser degree, were bound to remain specialised, if not exclusive, studies. We have had many occasions to point to the mystifications of the ancient inquiry into nature, but of course that is a feature that is still with us today, and one whose threat has increased immeasurably with the increasing remoteness and specialisations of science—as one might say of the massive superstructures that have been erected on or, rather, built over the foundations laid by some ancient visionaries.

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